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## Science Policy Study—Hearings Volume 8
## SCIENCE IN THE POLITICAL PROCESS

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ORIGINAL

# HEARINGS

BEFORE THE

## TASK FORCE ON SCIENCE POLICY

OF THE

## COMMITTEE ON SCIENCE AND TECHNOLOGY HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

FIRST SESSION

JUNE 25, 26, 1985

[No. 47]

COMPLETED

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Printed for the use of the
Committee on Science and Technology

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# SCIENCE IN THE POLITICAL PROCESS

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TUESDAY, JUNE 25, 1985

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
*Washington, DC.*

The task force met, pursuant to notice, at 10:03 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Fuqua. Today, our Task Force on Science Policy takes up the issue of science in the political process. This subject is one of the more difficult and sensitive issues in our agenda and also, probably, one of the most important.

It is difficult because no clear-cut answers appear to exist; it is sensitive because we as Members of Congress, along with our colleagues in the House and the Senate, are, ourselves, participants in the political process and thus potentially in positions to influence the outcomes of legislative proposals affecting science and its important work, because it has in recent years been the subject of intense debate, and because future trends in this area can have a profound effect on American science.

Since the Federal Government began to expand its support for scientific research following the Bush report in 1946, a tradition of leaving priority setting within science to the scientists has taken hold. That tradition is based on our recognition that only rarely do legislators have the knowledge and understanding to make informed judgments about priorities within science, especially when detailed knowledge about the contents is required.

This tradition has served us well. Along with generous funding, a strong university system, and many other factors, it has contributed to the emergence of American science into a position of preeminence in most fields.

But the tradition of leaving priority setting within science to scientists is just that, a tradition. Whether it should be continued or not and whether it should be followed in all cases is the issue before us.

Another great tradition which has helped build a strong science enterprise has been the partnership between science and government. As in any partnership, many things in the government-science partnership have worked well because both sides recognize the need to work together and to accommodate the needs and desires of both partners.

While scientists naturally want to see the partnership function in ways that advance science to the maximum extent, politicians must look to the broader national objectives to be served and must be mindful of the other factors apart from science which contribute to the achievement of those goals.

Some scientists have occasionally suggested that Members of Congress, when making unwanted decisions about science, allow themselves to be influenced by what has been termed "narrow political considerations." However, from the point of view of most legislators, the consideration, in setting priorities, for science of other factors such as defense needs, national and regional economic needs, and educational policy is not only desirable but an integral part of our job.

Thus, the issue before us is really, in my view, how decisions involving both science and politics should be made. The examination of that issue, which we begin today, will be carried out with great care.

The task force will soon issue a background study on this subject entitled "Expertise and Democratic Decisionmaking." Compiled for us by the Congressional Research Service, this collection of selected readings will help us place this issue in a much broader context, as suggested by its title.

To help us in our consideration of this important topic, we have a small but highly select group of witnesses who will appear before the task force today and tomorrow. We thank them for their willingness to share their experiences and thoughts with us, and we look forward to their testimony.

Our first person will be Dr. Thomas L. Haskell, Department of History, Rice University. Dr. Haskell, we are very delighted to welcome you here this morning.

### STATEMENT OF DR. THOMAS L. HASKELL, DEPARTMENT OF HISTORY, RICE UNIVERSITY, HOUSTON, TX

Dr. **HASKELL.** Thank you very much.

Chairman Fuqua, members of the task force, ladies and gentlemen, I very much appreciate the opportunity to speak before you.

I have been invited to speak not about the "brass tacks" of the very practical and concrete decisions you have to make. I will not offer any advice about that. I am here instead, as I understand it, to speak about the problem of peer review in a rather general, even abstract, way, the "high theory of peer review," if you will.

The agenda for this session sets out three questions. Let me quickly paraphrase them: How can the judgments of scientists and Members of Congress be effectively integrated? At what levels should decisions be made by scientists; at what levels by Members of Congress; and at what levels should some joint decisionmaking process be employed? And under what circumstances should science policy be shaped by perceived social needs such as regional economic growth or campaigns against certain dread diseases, for examples?

Underlying all of these questions, it seems to me, is another, more basic, question. Let me try to put it into words. In the formulation of science policy, when should the Members of Congress defer to the judgment of the spokesmen of the scientific community, and when, on the contrary, should the express preferences of the scientific community regarding science policy be overridden by Members of Congress, acting in their perfectly legitimate and ordinary roles as statesmen and as representatives of particular constituency in what is, after all, an interest group system of politics?

This question arises with a special severity in the work of this committee for the obvious reason that the work of this committee constantly goes back and forth across the intersection between two kinds of authority, two quite different kinds of authority.

On the one hand, there is that kind of authority that comes from winning a majority vote in an election for public office. On the other hand, there is that kind of authority that comes from membership in a community of experts whose knowledge has won the respect of the general public.

Now, the people who embody these two different kinds of authority occupy very different positions in life and see the world from very different vantage points. Their differences can at times be so great that they seem to be in different worlds.

And yet I think it should also be observed at the very outset of these discussions that as far as I can make out from news reports and from the material assembled by the task force for its background publication, neither party has any general interest in usurping the traditional prerogatives of the other.

Scientists, for their part, I think, are for the most part quite aware that their desire for ample funding and autonomy in deciding how to spend those funds must always be balanced against the need for accountability to the public interest. And I think most scientists would also be willing to concede that, in defining the public interest, the Members of Congress have a privileged voice.

And, on the other side of the fence, I think most politicians understand full well that science is a rather fragile and complex enterprise, one that develops according to a logic of its own and cannot be forced except in a moderate way, at certain times, and certainly no politician in his right mind wants to be seen as the fellow who killed the goose that lays the golden eggs of science and technology.

Each party then, I take it, concedes that there is a sphere within which the other's authority ought to reign supreme. The only question is where the boundary lies between these two spheres. That is, needless to say, an extremely difficult question, and one to which I can offer no very concrete or black-and-white sort of answer. Anyone who does offer a clear answer is someone, I think, who ought to be distrusted.

Though I cannot promise to deliver any exact or concrete criteria for making these decisions about when you are within your proper realm of authority and when you are in the other realm of authority, where you ought to defer to the other party, I think that the boundary area between these two forms of authority can be explored and might best be illustrated by me today by examining a few of the classic conceptions of just what science is and where the scientist's authority ideally derives from.

The quickest way I know to get to the heart of that question—the very nature of science and its authority—is to examine the conception of the scientific community advanced a century ago by the philosopher Charles Sanders Peirce. His stress on the conception of community is one that I will emphasize for reasons that I think will become clear by the end of my comments.

Peirce, of course, is thought by many to be the finest philosophical mind ever produced in America. He is one of the founders of that school of philosophy known as pragmatism. Although he is less well known than William James and John Dewey, the other two major founders of that school of philosophy, I think it is frequently argued that he is the more original of the three.

Charles Peirce believed that the very possibility of attaining truth depended upon transcending oneself and entering into intensely communal relations with other competent investigators. As if to repudiate Ralph Waldo Emerson's romantic advice to "trust thyself," Peirce contended that no individual, least of all oneself, could ever be worthy of trust.

"The individual man"—I am quoting Peirce now—"since his separate existence is manifested only by ignorance and error, is only a negation." Peirce's advice was to trust instead the community of inquirers.

Peirce's peculiar horror of idiosyncrasy and very strong preference for communal opinion was based upon his conception of reality and how mankind gains access to it. "What anything really is," wrote Peirce, "is what it may finally come to be known to be in the ideal state of complete information.

"Since information cannot be complete in my lifetime or yours, our best conceptions are riddled with error, and the truth can only be known by the last survivors of a community of inquirers that includes the yet to be born as well as the living, and that extends indefinitely into the future.

"The real, then," said Peirce in a famous passage that lies at the very heart of his philosophy, "is that which, sooner or later, information and reasoning would finally result in, and which is independent of the vagaries of me and you.

"Thus, the very origin of the conception of reality," he went on, "shows that this conception essentially involves the notion of a community without definite limits and capable of a definite increase in knowledge." The word "community" he had printed in bold-face type.

Now, Peirce was an eccentric man, and it may well be his status as an outsider who never found permanent employment in the academic world that sensitized him to the social consensual quality of all that passes for truth among human beings. Yet, there is no trace of cynicism in his conception. The ultimate consensus to be reached by his community of inquiries of a special kind, and his theory of reality, although it is indubitably social, is not at all relativism, as 20th century analogs to it have tended to be.

Peirce was a philosophical realist. He supposed that the universe was so made that an ultimate convergence of opinion about its nature was virtually predestined, and that the reality toward which opinion converged was utterly independent, not of thought in general but of what any finite number of human beings thought about it.

For him, reality was socially discovered but not at all socially constructed. When pressed by a critic, he allowed that the ultimate convergence of opinion might be incomplete in some matters and that the convergence was a hope rather than an inevitability.

But he insisted that the hope was of the same utterly indispensable character as the expectation of survival that a man struggling for his life must feel. To live is to hope, for such a man. Similarly, for an inquirer, a scientist, an investigator, for such a person, to inquire is to suppose that opinions ultimately converge toward the real.

The following extended quotation catches the spirit of Peirce's discussion of the community better than any other I know. These are his words: "The activity of thought by which we are carried, not where we wish but to a forward-aimed goal, is like the operation of destiny. No modification of the point of view taken, no selection of other facts for study, no natural bent of mind, even, can enable a man to escape the predestinate opinion.

"This great hope"—here is where he originally wrote the word "law"—"this great hope is embodied in the conception of truth and reality. The opinion which is fated to be agreed to by all who investigate is what we mean by the truth, and the object represented in this opinion is the real. That is the way I would explain reality."

Now, in order to see what Peirce was up to, I think we have to make a distinction between the strict meaning of what he said and the tacit implications of his doctrine. Strictly speaking, his theory of reality seems to make truth totally inaccessible to all living human beings, thereby opening the door to universal skepticism and doubt about all truth claims indiscriminately.

After all, the opinion to which he will assign the label "Truth" with a capital T is the one that will be held by the yet-to-be-born members of a future community of investigators. Peirce's Truth, then, never exists in the present; it always lies in the indefinite future.

For those of us who live in the present—as, goodness knows, we all do—this is quite literally a useless kind of truth, for no one can claim to know the final opinion of a community that extends into the indefinite future.

Yet, if Peirce's theory seems to court radical skepticism when strictly interpreted, it is reassuringly commonsensical when loosely interpreted; and how can we avoid interpreting it in this looser sense?

Once we accept Peirce's identification of truth with a community's striving, then if a community of inquiry exists in the field that interests us, it is difficult to resist the implication that that community's current best opinion is, in practice, the closest approach to the truth that we can hope for. Identifying truth with the community, but lacking the community's final opinion, we are bound to prefer its current best opinion to a chaos of indistinguishable truth claims, which is the only alternative Peirce's line of reasoning leaves us.

Now, if we compare Peirce's conception of science with that of modern writers, we find one point of pronounced similarity and another point of pronounced difference. The difference is that few philosophers today would be comfortable with what someone called the "naive realism" of Peirce's conception. He wrote at a time when many philosophers spoke about reality as if it were immutable and objective, something that exists out there, utterly independent of our perception, awaiting our discovery of it, much as the Western Hemisphere awaited the discovery of Columbus.

Some practicing scientists continue to talk about reality that way, but few philosophers of science would do so. The century that has elapsed since Peirce wrote has seen the rise of a much more complex and relativistic conception of reality, one that stresses the active intervention of mind against the meaningless flux of things and allows much greater room for change, for subjectivity, for legitimate differences of perspective.

In another respect, however, Peirce's conception of science is strongly continued and reinforced and endorsed by modern thinkers. Whether we look at the work of Thomas Kuhn, Karl Popper, Stephen Toulmin, or others who could be named, virtually all modern commentators stress, just as Peirce did, that science is a distinctly collective enterprise, that communal relations among its practitioners are not merely incidental but essential; that in fact they virtually define what science is.

Thomas Kuhn, for example, whose book, *The Structure of Scientific Revolutions*, is one of the most influential works of the last quarter century, employs a conception of reality that is far more relativistic than Peirce's, but he fully agrees with Peirce's stress upon the communal character of science; and what is more, he attributes to the community of scientists a truth-establishing, a truth-creating power, even, that is only slightly inferior by that ascribed to it by Peirce.

Kuhn regards science as essentially a puzzle-solving activity—a puzzle-solving activity carried out by tribes of practitioners who are drawn together into a community by their shared acceptance of a paradigm, a major scientific discovery or achievement that is rich with implications about what nature is and how it works.

The puzzles that scientists normally solve are generated by the paradigm they share and by their own competitive efforts to outdo one another in teasing out the implications of those paradigms.

Let me briefly read a passage or two to suggest the character of Kuhn's understanding of the community. "The very existence of science," Kuhn writes, "depends upon vesting the power to choose between paradigms in the members of a very special kind of community. Just how special that community must be if science is to survive and grow may be indicated by the very tenuousness of humanity's hold on the scientific enterprise.

"Every civilization of which we have records has possessed a technology, an art, a religion, a political system, laws and so on. In many cases, these facets of civilization have been as developed as our own. But only the civilizations that descend from Helenic Greece have possessed more than the most rudimentary science. The bulk of scientific knowledge is a product of Europe in the last four centuries. No other place in time has supported the very special communities from which scientific productivity comes."

He goes on to specify, among the many criteria of this kind of community, that one of the strongest, if still unwritten, rules of scientific life is the prohibition of appeals to heads of state or to the populace in large in matters scientific.

By the same token, he says that in matters of paradigm choice—that is, the most fundamental decisions the scientist makes—there is no standard higher than the assent of the relevant community.

The last thing I want to say about the scientific community is to stress its competitive nature, for it is this, I think, more than anything else, that justifies the kind of authority that scientists and scholars properly can be said to possess.

It is easy—all too easy in our day—to be cynical about the motives of professional people. We live in a period during which all professional communities—those of doctors, lawyers, any other profession you could name, as well as scientists—are likely to be viewed simply as monopolies, conspiracies by a band of insiders to exclude competitors and maximize their own income at the expense of the public.

There is much truth in this allegation, for professional communities do enjoy a kind of monopoly over the service they provide, and they can, and often do, extract monopoly profits. Setting decent limits to the incomes of professionals is one of the crying social problems of our day, and one that receives all too little attention.

But the monopoly model fatally obscures what I think is the most distinctive feature of a healthy scientific or scholarly community. What it obscures is the intensely competitive relations that prevail within such a community. When we hear the word monopoly, we think of a sheltered preserve within which competition is suspended. But this grossly misrepresents the character of a community of experts.

Such communities are made up—ideally, at least— of critics, people whose principal pleasure in life consists of showing that they can solve puzzles better than their peers. What they are competing for in this very competitive situation that goes on within each of these communities is not money—at least not in the first instance—but the effective currency of criticism: fame instead of disgrace; honor instead of shame; compliments rather than complaints about the technical worth of one's work.

Scientific communities are, in one sense, monopolies, then, but they are, more importantly, the very opposite of what the word monopoly leads one to expect. Monopolies, after all, aim to maximize pecuniary gain by minimizing competition among insiders through the exclusion of outsiders.

The scientific community, instead, deliberately intensifies competition among insiders in nonpecuniary dimensions of achievement such as glory and reputation in the solving of the kinds of puzzles created by the community. It is an arena of competition in which each competitor strives to accumulate not capital but reputation, a stock of favorable impressions of himself and his work in the minds of his peers.

Scientists and professors are among the most status- and prestige-conscious people in modern society, and here is the reason why: each, by virtue of his occupational situation, is caught up in a struggle for eminence based upon demonstrations of his ability to solve the kinds of puzzles that are generated by the very intense, fast-paced debates and discussions that are characteristic of professional communities.

The pecking order of these communities is taken with exceptional seriousness by their members because it presumably affects the outcome of the puzzle-solving competition that is of paramount importance to all of them.

Now, this is not to say that science is a pure meritocracy in which status is perfectly geared to merit. Clearly, this is not the case. This is a human institution. Like all other human institutions, it falls short of any ideal we may articulate for it. Frauds and fakes sometimes rise to the top. People with large reputations don't always deserve them. Peer review processes sometimes do, indeed, function like old-boy networks.

All of these things, however, I think, can be regarded as the kinds of imperfections that are unavoidable in any kind of human institution, and I confess that my own experience with these communities over the last 15 years of my professional life leads me to be generally persuaded that the competition is authentic and that the people who rise to the top of it generally are rather good; better than those who do not.

In conclusion, let me try to come as close as I can—which will not be very close—to the task of drawing out of these very general comments some somewhat more specific criteria that might guide the kinds of practical decisions that you have to make.

I can think of three situations in which you, as Members of Congress, risk doing serious harm to science if you do not defer to the judgment of the community's own spokesmen.

In the first place, if the scientific task to be performed is a given, and the question is who can best carry out this task, the community's own judgment, it seems to me, should almost always prevail. Only the members of the community know what the pecking order is. The fact that they will not fully agree about what the pecking order is and who can best do the job should not obscure the fact that there will be a fairly good consensus most of the time on most issues in matters of this kind.

Second, when the question is which of various scientific projects ought to receive highest priority, the judgment of the community's spokesmen again should prevail, I think, most of the time, for only members of the community can say where the growth points are given the current state-of-the-art.

The members of the community should not, I think, however, have full control over this question. Science does, indeed, have an inner logic of development, and no amount of wishful thinking, even when it is backed up by vast amounts of money, will produce a breakthrough when the proper conditions are not in place. All supporters of the Star Wars enterprise would, I hope, attend to that danger.

In this kind of question—Where can science be made to grow most rapidly?—the scientific community and the Members of Congress, I concede, need to exhort a kind of veto power over one another. Science should not be simply allowed to go its own way, according to its own inner logic, so far as the general public should not be obliged to pay for just whatever scientists want to do. But when Congress tries to say what science shall become, it treads, I think, on very shaky ground.

Third and last, I think Congress will always do harm when it distracts the members of the scientific community from the puzzle-solving activity that constitutes science. It will also do harm when it dilutes the force of that competitive activity by encouraging other kinds of competition within the scientific community.

Scientists should rise or fall by their ability to solve the puzzles that the community generates through its debates and technical communications. When their status is influenced by other kinds of performance—when, in other words, they are made to compete in other dimensions of skill—science is the loser.

Let me suggest a very homely analogy. Imagine that it is your responsibility to field a basketball team to go to compete in the Olympics. What you want is people with a very specialized skill—the ability to put balls through hoops and prevent others from doing the same thing.

In order to choose the best team, it is crucial that you narrow your criterion as much as possible to that very specific kind of skill. What you don't want is a team selected on any other basis such as the congressional district in which the player resides or his skill in hiring lobbyists or managing a bill on the floor of Congress. These are different kinds of skill; they involve different dimensions of competition; and they are not conducive to science.

I would close, then, by urging you to do everything in your power to avoid generating competition among scientists that rewards any skill other than that puzzle-solving activity.

Thank you very much.

## DISCUSSION

Mr. **Fuqua**. Thank you, Dr. Haskell, for a very interesting discussion.

I might point out—you were saying scientists are very competitive, and I guess that maybe is the way you would describe politicians, particularly Members of Congress, because none of us get appointed to these jobs; we have to aggressively seek them, sometimes maybe against our better judgment, but we are in there fighting. So maybe that puts us on an immediate collision course.

Dr. **Haskell**. That is where the problem is.

Mr. **Fuqua**. And I might point out that we have a constituency that depends on us to look after what they perceive as their interests, and there have been attempts—we have seen Congress establish mission-oriented institutes, like in the NIH, for arthritis, nursing, cancer, some of the others, with a large constituency. Are there any lessons that can be drawn from that? That was at the opposition of the medical community.

Dr. **Haskell**. Right. I concede that, in that particular case, since my mother and sister both suffer from arthritis, I had a hard time resisting the logic of your position. I think it is the murkiest of the areas that I mentioned and the one in which some sort of integration, some sort of check and balance between the two different kinds of authority, is most essential.

I, for one, cannot, in that particular issue, suggest that there is any neat rule to follow. All I can suggest is that the competition go on, with a due regard for the legitimacy of the other's authority, which I know does not solve your problem.

Mr. **Fuqua**. Nobody said it would be easy.

Dr. **Haskell**. That is right.

Mr. **Fuqua**. Also, when you were speaking of this all-star team, you know, when amendments are offered or maybe certain traditional procedures are circumvented, none of these originate in Congress. Most of the time someone asks you to do that, or you are approached by the university or a certain group asking that this program be supported; we could do a good job with it. And so it gets right back into this circle. Could you comment on that?

Dr. **Haskell**. Yes. I think there is immense danger from precisely that source in that the members of the scientific community are not bound together by any oath of solidarity in matters like this. Precisely because they are so competitive, they will be very eager to accept any opportunity that they get for advancing their research and getting the funds that it takes to advance their research.

What I fear there is that unless that competition is kept within fairly narrow channels and ones that, to the greatest possible extent, rely upon this special kind of competition that I think goes on within the community, unless that happens, if the floodgates of competition are opened in general to other kinds of competition, that the kinds of people who rise to the top in science will not be those who are best equipped to do the real work of science.

Frankly, this is a truly idealistic position and will seem wildly unrealistic to you, but I have grave reservations about the whole grant system as it already exists on that score. I think there is real merit to the claim that grantsmanship has come to count for more than scientific insight. This is, I think, an acceptable kind of——

Mr. **Fuqua**. Let me query you further about that. There have been those that said that the peer review process —you mentioned that—is probably just a further extension of the "good old boy" network, and that the big, more traditional schools, the older, prestigious schools tend to have the people on the peer review, and they are reviewing their own peers; and the emerging institutions that have, maybe through their States or private funding, have put considerable resources in recent into upgrading those schools, having better faculty, better facilities and so forth, and yet, because they have not been involved in physics for 40 years or 100 years in this particular area, or astronomy or whatever it might be, that they are somehow inherently unqualified to get these.

How do you respond to that? It is a little bit like the seniority system in Congress. It has a lot of faults. I don't know what you replace it with, and the same with the peer review system.

Dr. **Haskell**. I think you arrived, with that last statement, at the conclusion toward which I was going to head. I think that the peer review system is flawed. It is susceptible to all these abuses that you named.

My own experience—again, I have to simply testify from personal experience—is that, on the whole, it has worked rather well. I teach at an institution that is not generally considered to be in the first rank, Rice University. Rice would ordinarily be ranked in about the second level, perhaps.

And yet I think that I would be willing to concede that, on the whole, the rankings within my profession correspond fairly well with real merit; and I certainly have felt that, within my profession and the ones that I know something about—and I do know something about ones other than history—the effort that everyone makes to make judgments in the blind—blind, that is, with regard to the institutional attachment or personal friendships and matters of that kind—people really do bend over backward to do that, for the most part. They do not always succeed, as you would predict, and there are some who don't even try. But I think, on the whole, the system really does work rather well, and that the pecking order is a reasonably close approximation to genuine merit; always, finally, with the understanding that there are flaws but that no other system that I know of would do any better at arriving at a judgment of where the best talent lies.

Mr. Fuqua. You mentioned grantsmanship, and that has certainly been one of the arguments and criticisms that has been made, I think legitimately, of our present method.

I think it was Dr. Press, Frank Press, who is now the president of the National Academy of Sciences—and if it was not he, I apologize for attributing it to him—but I think it was Dr. Press who stated that probably we should consider not totally abandoning peer review, but that we look at not so much what you write down that you plan to do, but what have you done in the last 3 to 5 years in the way of research; what types of graduate students have you produced; what generally is your track record; and let it be based on that rather than your ability to write a grant. What do you think of that?

Dr. Haskell. I would think that it was indispensable to a successful peer review system to balance these two kinds of considerations. Track record is not always a good indicator of what a person is about to do. He may have run out of good ideas. He may be in a dead spot. He may be beyond his creative phase altogether.

By the same token, a bold and smashing project in description has to be weighed in terms of the believability of it, in terms of the person's talents and abilities as revealed by his past record. So it seems to me you cannot choose between those two; you have got to employ both.

Mr. Fuqua. That could work to a disadvantage to young researchers that are just coming into the academic community that have not really had a track record. How do you get experience without getting experience?

Dr. Haskell. Well, I think the force of a well written proposal that strikes to the heart of a problem that the community has come to recognize is sufficient that a young person, although he does face a greater barrier than an experienced one—I will grant you that—doesn't face an insuperable barrier. The barrier is only as high as that absence of a track record makes it, and a well written proposal will generally persuade the reviewers.

Mr. Fuqua. Mr. Brown.

Mr. Brown. Dr. Haskell, I would like to compliment you on your presentation. I think it was quite provocative, and it brings a dimension to our examination of science policy that I think we probably need to dwell more on than we have in the past.

Let me briefly discuss with you the peer review system, which is one of the critical elements in science policy. We have at least two systems for allocating funding for scientific research and development. One of them we generally refer to as a peer review system and one we refer to as a formula grant system, in which we just allocate so much money to laboratories and assume that they will do the research that they should be doing, both basic research and applied research.

I would like to pose this thesis and have you comment on it, and that is that there are values to both kinds of systems, and those values are based upon a rather pragmatic analysis of results; that a peer review system can become encumbered with grantsmanship; that a formula grant system can be encumbered with old fogies who are no longer in touch with the community; and that both need to be corrected.

A formula grant system needs to have its work periodically reviewed, the scientists or the laboratories, by peers who can help it to keep on course and thus provide for a fruitful use of the public money.

Likewise, peer-reviewed systems need to be reviewed themselves to determine whether the peer review groups are representatives and not just old boy networks, and whether they are getting to spend too much time—and I have heard criticisms that half or a third of their time is going to preparing grant requests—whether this is getting out of hand.

So, in both kinds of cases, we need to examine this from a pragmatic viewpoint as to how we achieve the most productivity and the best puzzle solution. Therefore, it is not really a question of just blessing peer review but a question of evaluating results. Could you comment on that?

Dr. **Haskell**. I confess that I am simply not knowledgeable about especially the formula grant system. I have no personal experience with it.

Mr. **Brown**. That is the oldest system we use. That is the one that the Department of Agriculture uses, for example, very largely.

Dr. **Haskell**. And I take it that its principal feature is that money is simply assigned to an institution which then allocates distribution of it?

Mr. **Brown**. It is distributed to the States under a formula, and the States use it for State-supported agricultural research. There is a land-grant college in each State that benefits from this formula distribution.

Dr. **Haskell**. I see what you mean.

Well, I again must, I guess, beg off that. I simply don't know enough to really draw on any conclusions on this, but I will say that I know, among the students of professionalization, the money doled out to the land-grant colleges is a kind of classic illustration of the fact that when money is available, researchers will appear to do research, and in spite of their appearance, there is no guarantee that science will progress simply by virtue of the number of researchers involved in something.

I don't mean to say that scientific research has failed in agriculture. Clearly, the United States enjoys its supremacy in agricultural production in part because of its fine agricultural science. But I do think that the amount of money available for agricultural research probably exceeded the capacity of science to employ it fruitfully at some stages, at least. I wouldn't say that necessarily now, but at some points—and how you prevent that, I do not know, except by keeping close tabs with the community of scientists on where science is actually prepared to grow at this particular moment and where it is not.

Mr. **Brown**. Well, we have to deal with the problem at a grosser level than this, actually. Even in the finest peer-reviewed science, such as what we have, we will say, from the National Science Foundation, we always structure that process by saying that there is so much money available for this program. It may be that it is high energy physics, or it may be some other program. But the amount of money available, in effect, attracts researchers to that field.

Dr. **Haskell**. Right.

Mr. **Brown**. And those fields that do not get funded—like history, for example—you don't get as many.

Dr. **Haskell**. Our work is cheap. [Laughter.]

Mr. **Brown**. I know it is cheap.

Dr. **Haskell**. It doesn't cost much.

Mr. **Brown**. But that doesn't mean we don't need it. I am beginning to think that history is the most important of the policy sciences and the one that the Congress needs the most of right now.

Dr. **Haskell**. I couldn't disagree with you.

Mr. **Brown**. You would agree with that, of course. [Laughter.]

Dr. **Haskell**. I would be happy to agree with you, yes.

Mr. **Brown**. Let me go back just briefly. I don't expect to get much out of this, but you spent quite a bit of time discussing Peirce and his concept of reality as being grounded in a consensus of the community.

This is not true just of scientists; this is true of philosophy, religion; you can probably name a number of fields. The important thing—and I am just verbalizing here—it seems to me is to recognize that there isn't any such thing as a reality that can be achieved, but what you get from a community is a perception of reality.

Dr. **Haskell**. Yes, I think that is increasingly the position that commentators on this subject have taken; Thomas Kuhn's book, *The Structure of Scientific Revolutions*, for example.

Mr. **Brown**. Correct. He is slightly advanced over Peirce from that standpoint. We have these perceptions which become embodied as paradigms, as you commented.

Within a particular paradigm or perception of reality, there is this intense competition which is mediated by the consensus within the community, but generally speaking, when you get a paradigm shift, as Kuhn describes it, you have a renegade of some kind who does not depend upon the community.

In that sense, the basic benefit of science is encouraging renegades who do not abide by the consensus or the perception of reality of the particular community. How do we weigh that in the equation?

Dr. **Haskell**. It is a very interesting problem, and there is one commentator on this subject, Feyerabend, who would carry the argument that I think you want to see extended a little farther and argue that the stress on community in science is wrong and that good science should be understood to be a matter of bold, radical innovation in which the individual's insights owe nothing to the community within which he operates.

I am not persuaded by this point of view. I think that Kuhn's stress on the community is much closer to the mark, and in fact I think we would not want always, in every case, to support the renegade.

When a genuine scientific revolution takes place—that is to say, when a new paradigm is set before the members of a community and it wins their allegiance, or wins the allegiance of a majority of those who are active and fruitfully engaged in the work of the community—then I think it ought to triumph, and the more rapidly, the better.

If Kuhn is right, that is the only kind of progress that science ever could or can make. To support the renegade in a situation like that, the man who clings to the outworn paradigm, would hold science back rather than advance it.

But what you, I take it, mean to stress is that, after all, conformity is not an admirable thing, and all this talk about the virtues of consensus in a scientific community comes very close to saying that conformity in these communities is a good thing and that the individual is somehow to be suppressed.

I acknowledge that there are real difficulties with that. I think that when Kuhn talks about the triumph of a consensus, he takes it for granted that nature is such [illegible] consensus will not form unless the paradigm in question is [illegible] adequate to the reality that it purports to deal with; and [illegible] consensus for that reason deserves our support and our loy[illegible] that every consensus, after all, begins as an individual insight, [illegible] if an insight remains purely individual, if it remains purely idiosyncratic, that is a sign that it is somehow not adequate to the reality that it purports to deal with.

I am not sure I answered your question in that.

Mr. Brown. No, but we have had an interesting discussion, I think. [Laughter.]

Dr. Haskell. Thank you.

Mr. Fuqua. Mr. Lewis?

Mr. Lewis. Thank you, Mr. Chairman.

Most of us in Congress and other areas of the political arena are constantly lobbied by the lobbyists of various interests and various influences. Now, where do we draw the line to make a determination, as in your presentation? As I understand it, you made some three areas. One was that lobbying interests should not influence; the scientific interests should be the influencing factor.

Dr. Haskell. Well, that is a question of brass tacks, and I am not familiar enough with the mechanics of the political situation on the floor of Congress to really be able to give you a good answer to it.

But my general answer is that I think it is much to the advantage of science and to the long-run advantage of the public if scientists be kept out of direct competition for the kinds of benefits and advantages that are doled out on the floor of Congress. To the extent that scientists can be excluded from that, I would be pleased. I think it is to their benefit.

What that means, then, is that other kinds of channels need to be established or revived, it seems to me, that would permit those decisions not to be made on the open competition of the floor of Congress, but for the decision of who is to get the funding for which project, to be made in a way, that is much more closely geared with members of the community, who know who is who, and what kinds of projects will move at a given time.

Mr. **Lewis**. Would you feel that this ought to be done by Members of Congress who have the most influence over the issuing of the grants or the financial abilities to smaller institutions? I have a feeling that the smaller institutions are left out and sometimes are as good as, if not better than, the major institutions throughout the country as far as their science programs are concerned.

Dr. **Haskell**. I think there is some truth to that. I am not at all confident, though, that the smaller institutions would fare any better on the floor of Congress. I think what would fare better on the floor of Congress, were the decision to be made more and more in that arena, would be those institutions, big or small, that managed to put together the kind of clout, with lobbyists or whatever, that it would take to carry the decision there.

My only point is—and I don't mean to derogate this kind of skill and ability; it is a very real and authentic one—but it is not the kind of talent that ought to determine the pecking order within the scientific community, and to the degree that the scientific community is forced to change its style and engage in that kind of competition, I think there is real damage threatened to the community.

Mr. **Lewis**. Do you feel that in all areas, science and otherwise, the decisions are based on political interests to make the grant awards in relation to the ability to perform a particular act?

Dr. **Haskell**. I am sorry. Do I think that within the scientific community, decisions are made on the basis of merit?

Mr. **Lewis**. Are we shirking the scientific community overall because of political interest and the influence of political interest in making the decision as far as awarding grants?

Dr. **Haskell**. My sense of the present situation is that, on the whole, Congress has been remarkably and admirably tolerant of the prerogatives of the scientific community to make crucial decisions about where the money will go and what it will be used for.

What I mean to advocate is a continuation of pretty much the same situation that now prevails. I would only be alarmed at a move toward a greater politicization of the decisionmaking process.

Mr. **Lewis**. Thank you. Thank you, Mr. Chairman.

Mr. **Fuqua**. Mr. Lundine?

Mr. **Lundine**. I don't have any questions.

Mr. **Fuqua**. Thank you very much, Dr. Haskell, for being with us this morning.

Dr. **Haskell**. Thank you.

[Answers to questions asked of Dr. Haskell follows:]

## QUESTIONS AND ANSWERS FOR THE RECORD

### Dr. Thomas L. Haskell

1. **Have there, to your knowledge, been comparable cases in other fields, such as law, medicine, defense policy, social security, and welfare policy, where the balance between professional judgment and political judgment has been at issue?**

   The "Red and Expert" controversy in China during the Cultural Revolution is the most extreme, and therefore illuminating case of conflict between professional and political judgment that I know of. The central question was how far the Party could recognize (or tolerate) forms of authority and expertise other than that constituted by party membership itself. At times the attack on technical experts, trained cadres, specialists -- anyone claiming authority that originated outside the Party -- became so intense as to threaten their annihilation.

2. **One of the traditional rules of the political world, arising from the concept of representational government, is that legislators have a duty to look out for the interests of their constituents. This a particularly strong tradition in our American system where we have no members elected "at large" or on the basis of total party votes, but all represent specific geographic areas and the people living within that area. Should we expect legislators in such a system to exempt certain parts of the Federal Government's activities, be it science, or defense, or the arts, or any other field, from such political interest of influence?**

   We most certainly should expect legislators in our system of government to exempt certain activities, including science, from direct political influence. The subordination of all forms of authority to the political is the way I would define totalitarianism. The defining attribute of a pluralistic society is the comparative independence of science, scholarship, art, and many other spheres of life from direct political influence. It makes no difference whether the source of political authority is an arbitrary dictator or a democratic majority: to the extent that political authority is so pervasive as to exclude other independent sources of authority, the polity is totalitarian. Needless to say, the Constitution checks and limits the sway of majority rule at many points, so there are ample precedents in our system for preserving the autonomy of science, even as science becomes increasingly dependent on federal financing.

3. **If certain aspects of federal activity, such as science, are exempted from political determination, what happens to the concepts of political accountability for the individual legislator?**

   Total accountability might well require totalitarian measures, but a reasonable and democratic degree of accountability is compatible with the autonomy science needs in order to thrive.

Consider the nature of the legislator's role. No wise legislator regards himself as the mere errand boy of his constituents sent to Congress to advocate mechanically their most immediate, concrete interests. Winning election in our system of government confers upon the legislator a considerable amount of discretionary authority, authority to act as a statesman, a leader who takes a broad view of his constituents' interests and even looks beyond all private interests to the public interests when the occasion demands.

For example, loyalty to constituent interests does not entitle (much less require) any legislator to seek federal funds to build a harbor in his district if the local geographic conditions are unsuitable for navigation. By the same token, constituent accountability is no justification for trying to locate a research laboratory in a district where local personnel and institutions are incapable of putting the facility to optimum use.

The ratio of creative to uncreative people in science, as in most human endeavors, is perhaps one in ten. If Congress allocates research money to mediocre scientists, whose principle virtue is that they reside in someone's Congressional district, it can be sure of getting only mediocre science. The idea that first rate scientists will go wherever the money is, so that the identity of the recipients who initially administer the facility makes little difference, is utterly mistaken -- not because scientists are unresponsive to pecuniary incentives, but because mediocre scientists tend to hire people as mediocre as themselves, no matter how ample their budget.

There is no easy compromise on this issue: the legislator must choose either to support quality science or to spread the gravy around his district. He cannot do both at the same time, except in the rarest of instances. The legislator who trusts the judgment of the scientific community in the allocation of research funds, and overcomes the temptation to treat those funds as one more piece of pork in the barrel, is living up to the highest standards of statesmanlike accountability.

4. **Those who have expressed a concern about efforts to obtain research facilities by a resort to the political process have noted that some projects have been initiated not only without scientific review, but also, in the case of floor amendments, without review in the Congressional system; that is, through subcommittee and committee hearing and debate. Would legislative provisions for such facilities be more acceptable if they were the result of such a more extensive Congressional review?**

No doubt there is more than one way to bring peer review to bear on the allocation of research funds, and perhaps it could be done through committee hearings and the like. To me, however, this sounds like a cumbersome and unpromising approach. Certainly committee hearings would not serve the desired end unless they gave full voice to peer judgments.

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5. **In the event we continue to see the practice of lobbying for research facilities or other attempts to make judgments about science within the political process at about the level of the past 3 to 4 years, what, if any, will the long-term effects be, in your opinion? Is there a threshold level where this practice has serious adverse effects, in your view?**

   My guess is that if the current drill into pork-barrel allocations in science is allowed to go on for another few years, all universities will feel forced to enter the fray and the intensity of competition will rise rapidly. Neither science nor Congress will ever be the same.

6. **It is being argued that the Congress should provide general goals and guidelines for science programs, including science facility programs, and let the agencies of the Executive Branch do the actual selection of the institutions and individuals to receive support. Yet in other areas where the Congress provides support, most notably in the defense procurement and the defense construction areas where a high degree of expertise presumably also is needed, the legislative provisions are highly detailed ("$18 million for M-1 heavy trucks", "735,000 for a refrigeration building at Tyndal Air Force Base"). Is there any evidence that one system is notably better or worse, or are the different approaches mostly a matter of tradition and practice?**

   How could defense procurement practices be cited as a model for anything?

7. **Apart from the question of the respective roles of the members of the political world and of the scientific community, what means do we as legislators and you as a member of the scientific community have to "enforce" or at least encourage compliance with whatever policies or guidelines that we may arrive at in this area?**

   I do not think the academic world has any acceptable and effective means of enforcing the desired taboo on direct legislative appropriations for science. Retribution against offending parties through the peer review system itself, as some have recommended, would clearly be self-destructive. Only Congress has the power to keep the floodgates of this disruptive and immensely wasteful competition closed.

8. **Would it make any sense to establish a cost limit for science projects and facilities below which legislative specifications should not be made, but above which statutory specifications would be appropriate?**

   As a last resort, and as an alternative to the competitive scenario I have predicted above, I suppose that a measure especially ruling out direct legislation below a certain monetary level, and legitimizing it above that level, might be better than nothing, if the level were set high enough. This would be no more than a stop-gap measure, however.

9. Is it possible to reach a sufficiently strong consensus about the fairness and effectiveness of procedures for establishing priorities so that individuals and institutions that attempted to circumvent those decisions could be effectively censured by the rest of the community? What types of sanctions might be used to force compliance?

As I said above, I see little prospect of any spontaneous observance of a taboo against direct legislative action by those who stand to gain from violating it. Nor do I see how any sanctions applied within the academic world could be both fair and effective. The only hope is for a continuation of the admirable self-restraint that Congress had traditionally displayed.

Congress should treat the allocation of funds for scientific research the way it treats the selection of contestants for Olympic teams: let the decisions be made by those who are familiar with the field, because a team selected on the basis of competition between representatives of different congressional districts is bound to lose.

Mr. Fuqua. Our next witness is Robert L. Sproull, the chairman of the Working Group on Institutional Renewal, Government-University-Industry Research Roundtable of the National Academy of Sciences.

Dr. Sproull, we are very glad to have you with us this morning.

## STATEMENT OF DR. ROBERT L. SPROULL, PRESIDENT EMERITUS, ROCHESTER UNIVERSITY, ROCHESTER, NY, AND CHAIRMAN, WORKING GROUP ON INSTITUTIONAL RENEWAL, GOVERNMENT-UNIVERSITY-INDUSTRY RESEARCH ROUNDTABLE, NATIONAL ACADEMY OF SCIENCES

Dr. Sproull. Thank you, Mr. Chairman.

I feel a little embarrassed at this point. I am going to come down from the Olympian stratosphere to pretty practical considerations. I am afraid I am going to lower the tone of this whole enterprise a great deal here.

I have taken your invitation quite literally to respond principally to pages 55 and 56 in the committee's agenda. I am appearing here as a result of my service for the National Academy of Sciences/National Academy of Engineering in connection with the Government-University-Industry Roundtable, which has many of the items on your agenda on its standing agenda.

It is an attemp[illegible] see what a standing committee composed of people from universities, from Government, from industry can produce over the years. As I will come to later, we have interacted with you and your committee, and we intend to profit by additional interaction as much as we possibly can.

I also appear having spent most of my life in the university, but having spent 2 years in this town as the Director of the Advanced Research Projects Agency, so I have some experience of the other side of the problems.

I would prefer, with your permission, not to read my testimony but simply to go through and put notes and footnotes on it. I think you have it front of you.

Mr. Fuqua. Yes; and we will make it part of the record.

Dr. SPROULL. Thank you very much.

In connection with your question 1, "How can the expert judgments of the scientists and the societal goals-oriented judgments of Members of Congress effectively interact?" I answer a good deal of that in connection with No. 2 but also have responded to your sentence asking for cases. That is what most of the next few pages are.

On page 2, I say that the system has worked very well indeed, especially when it has involved individual research projects. It is not the only system. In the United Kingdom, one has the University Grants Committee, which has been the envy of some of us for some time. On the other hand, the United Kingdom is finding difficulties with that approach.

We also envy the Federal Republic of Germany, and particularly in its interactions between universities and industry. I think we have lots to learn from other countries, but in general, I think our system—the interaction of Congress, the agencies, the universities—has worked extremely well and has, if anything, been the envy of the world.

Later on page 2, I say that where the largest scale facilities are involved, then is when considerations enter the picture in addition to the promise and quality of the individual work.

The next few pages—I will not repeat them—simply recite, in response to the last sentence of your question 1, some of these cases.

I would like now to turn to the middle of page 6, where I address your second question: "At what level should decisions be made by scientists, by Members of Congress, and jointly?"

My first reply to that is that scientists should make no decisions whatsoever on the Federal support of scientific research. Scientists make the most important decisions with committing their own lives. When a scientist says that my curiosity and my talents take me in this particular direction, he is making a very important decision, and that is a decision that he can make better than any other person.

The sum total of those decisions is extremely eloquent in ultimately deciding on the way Federal dollars are spent. But I don't think that scientists by themselves should be making any decisions whatsoever.

On the other hand, as I go on to say, scientific evaluation of the promise of an individual research or facility proposal, I believe, is an absolutely essential part of the process; that that scientific evaluation is a part of the process; it is not the deciding part of the process.

Scientists in each field make important decisions: what field is most promising; how to balance the service to an individual student versus service to the next generation through research; how to balance industry support, which is small but growing, against local support and against Federal support. These are decisions that scientists individually can make and should make.

But when it comes to the size of a Federal program in, for example, high energy physics versus a program in support of, for example, chemistry or materials science, this seems to me, pure and simply, a congressional decision based upon the best advice it can get from the agencies and from the scientists, so that the size of programs, the funding of programs is something where scientists should participate as part of the evaluation process but something where they do not decide.

From here on, I sharply distinguish between individual research programs, on the one hand, and facilities programs on the other, because it seems to me my advice, anyway, diverges at that point.

On the top of page 7, I hate to sound like Pollyanna, but it seems to me there is nothing basically wrong in the system as it is, where the Congress approves R&D programs, establishes new programs, sets funding levels, and provides guidelines and objectives for the conduct of the program.

Then the executive agency works through this. It has advised Congress earlier, so the congressional decision is based in part upon agency advice. But then the agency makes the proposals compete one against each other; it makes the ambitions of one region compete against the ambitions of another; and ultimately, then, at least on the large projects, as I have said in some of the earlier examples, Congress ultimately can make the decision as to whether—for example, a new accelerator like the SSC, if it is ever authorized by Congress, I am sure Congress will be in a very central position in deciding where that is to go.

There is never enough money for all of the promising ideas. I think, although all of us realize that this produces agony among us—and remember that I was a university president for something like 16 years, and there was never enough money for everything to go around—before one weeps too many tears for his life, though, I ask you to imagine yourself in a country where this was not the case, where there was more money than ideas, and what a terrible, dull place that would be.

So we should not be too upset about the fact that there is never enough money to go around for all the promising ideas, because that makes one idea compete against another, and in general, I think we can all take some pride in the fact that in our country the best ideas have floated to the top.

This is really a tribute to the self-restraint on the part of Congress and the analytical capabilities of the agencies, the executive branch agencies, and the way that they have enlisted the scientific community to advise them.

The central feature, as I say on page 8, of all such reviews is the use of recognized experts, but by the agencies, within the framework set up by each individual agency to respond to the congressional intent in authorizing and funding the program, and this is the way it has worked within the individual research programs and, I think, worked very well.

I will come back, if you will, to this "old boy" business a little bit later, which is one of the accusations made about that system, but I think it is somewhat misunderstood.

At the top of page 9, however, I switch gears to facilities, and this arena has been the arena of rapidly increasing tension. The first thing I address myself to is why. Why is this an area of rapidly increasing tension?

I see, since I have just read this morning H.R. 2823, that I am now preaching to the choir on page 9, and so I will make that very short indeed. But I do want to emphasize that research facilities have deteriorated and are obsolete, and it is a really strong national problem, and I am very glad that you and your committee have risen to it.

The reason this got this way is that we were all encouraged, both by the Federal Government and by the States, in the postwar and the postsputnik period and the Great Society period, to expand, and in fact it was considered unpatriotic not to expand.

Now we are facing a situation where the enterprise is no longer expanding. The number of 18-year-olds is not growing; in the Northeast, it has even declined. Federal funds for research in real terms, although there are fluctuations, in a secular way, are not expanding.

The universities are left with an aging plant. The only way we have, at Federal expense, to replace that is by the 2-percent use charge, which is really a very bad joke. It assumes that an academic building lasts for 50 years. Well, the shell of an academic building may last forever, but if it is going to be used for research, it has to be completely renovated every 15 or 20 years at an expense which almost always far exceeds the price of the shell in the first place. So the universities have not had the ability to keep the facilities up to date.

Under four, at the bottom of page 9, I point out that to maintain research and teaching programs at the frontier needs modern facilities. It is even worse than that. We want to be fair to advanced students. Advanced students—Ph.D. students in universities—could be earning far more, particularly in engineering, if they took jobs and sometimes would be supported in advanced work by their industrial employer. They could be earning far more than they do as graduate students in universities.

We believe it is grossly unfair to take advantage of them by asking them to do that unless the facilities are absolutely up to the frontier of the science or engineering.

On page 10, I want to call your attention to item six, which we may get into in the questioning. I noticed Congressman Lewis pointed this out in his questions. The question is the unusual routes that some colleges and universities have taken recently.

I simply want to point out that when one of these routes is successful, it probably makes life tough for you Congressmen, because I am sure a lot of other people have come to you and asked for similar treatment. But it certainly makes life tough for us who are trying to replace obsolete facilities on universities.

Imagine, for example, my interaction with my board of trustees—which happens to be a private university, but it would be the same, I think, if it were the regents of a State university—when, in what we might call the pre-Columbia period and the post-Columbia period. Raising funds for academic science is very hard work. I don't know how much many of you have done on that, but it is very hard work.

Mr. Fuqua. Every 2 years. [Laughter.]

Dr. Sproull. Well, that may be even harder work, but I don't know anything about that. Everybody thinks his own field is the most intractable.

But when I go to my board of trustees now and say that we desperately need a new chemistry building, their answer is not quite this naive, but close to it: Why can't you be as smart as my friend, Mike Sovern, and go get the money from the Federal Treasury?

When the trustees have to, with their own money, in effect, seed the project and go to their friends to get money, go to industry to get money, any time that somebody has successfully, in effect, short-circuited that process and gotten what seems to be very direct access to the Federal Treasury, where the corporations and the individuals have already paid their taxes to support it, it just makes life very hard for the rest of us to go the more traditional route. So the post-Columbia world is different from the pre-Columbia world.

In the middle of page 10, I just want to say that although I am not here to endorse any particular Federal or other type of program for facilities—and I have had a chance to read through H.R. 2823 only once—but, nevertheless, I think I can warmly endorse it. I particularly endorse the idea of cost sharing and the idea of asking the National Science Foundation to, in effect, make a periodic audit of the state of facilities.

These are two aspects of it which I warmly endorse. But the main thing I endorse is that it, we hope, might create a Federal program, and I will come to that a little bit later.

At the bottom of page 10 and top of page 11, I say that the role of Congress in direct Federal support of facilities is the authorization of funding of programs. I distinguish that from individual projects. But the creation of programs is certainly a prerogative of Congress.

It also seems to me to be a prerogative of Congress to decide on what the program objectives should be and what criteria, if any, should be used in addition to scientific and technological merit and promise.

For example, although I don't think it is spelled out specifically in H.R. 2823—it might well be—my own view is that the number of square feet of academic facilities should not be increased in the period of the next decade or two. If the number of square feet is increased, it is just going to be more facilities that are going to be begging for support, and in a situation where the number of 18-year-olds is not expanding, where the likelihood of sharply expanding Federal support for research is not very good, it seems to me that just expanding the number of square feet, while not exactly wicked, is certainly not in the public interest.

That is to say, then, for any new facility that should be built, some old facility that was devoted to science and engineering should be taken out of service, either razed or converted to areas which do not compete for Federal research dollars; or—and this should be perhaps the bulk of the program—the dollars should go into modernizing existing facilities, perhaps continuing the shell but putting in modern plumbing and wiring and air handling systems so that modern research can be done.

Under two at the top of page 11, I point out that there are other ways of funding facilities. Loans, for example, interest-subsidized loans, can be managed by the Federal agencies, cost-sharing Federal funding of facilities, and so on, all where the original competition has been submitted to the agencies and with a comprehensive merit evaluation of discrete proposals by the agencies. This seems to me to be the essence of a sensible and promising program, and I am happy to see it in H.R. 2823.

To turn to page 12, the question of other factors of a more social, political, and economic nature, it seems to me these are taken into account now, should be taken into account. It is an area of particular congressional interest. But what I emphasize in my testimony is that the individual institution, the individual State, and the individual region have a very good opportunity to compete.

If it is more ambitious than other States or institutions or regions, if it is more confident that it is going to be able to assemble quality people in the future, if it is more irritated by what it calls the "old boy network," then it has a chance, with cost-sharing programs, to put its money where its ambitions are, and that is why it seems to me so important that the local ambitions and aspirations be given a chance to work in the process through cost sharing or through, for example, taking interest-subsidized loans rather than grants.

I emphasize that at several points in this testimony, particularly in the middle of page 13, where I talk about the various possibilities, including loans.

I want to emphasize that the agencies of the executive branch are already staffed to run such competitions. A competition usually ends with one friend and a great many enemies, and the agencies have adjusted themselves to that unpopular life. I am sure that you people can tell me more than I could ever imagine about how that works in your lives, where you produce one friend and a great many enemies by direct congressional action.

At the bottom of page 13 and the top of page 14, I address the question of, "To him that has shall be given," that type of thing. This is a difficult area and one where a great deal of tension has developed among the newer institutions, particularly, and the smaller colleges and universities.

It is one that there are not any good answers to. I just want to point out that the institutions like MIT and Stanford and so on who are the "haves" now got that way through a great deal of their own investment and a great deal of risk taking by their own boards of trustees and by generations of faculties and administrators.

It was not done easily. But, on the other hand, it was done in an era of expansion; and, in an era of expansion, it is a quite different situation. The Federal funds in the immediate postwar period were, quite appropriately, I think, a part of that expansion.

The situation is totally different now, where there is no expansion, and it means that if Federal funds are to be applied to bring a first-rate small college into the arena of research universities, for example, or a region of the country which has not had success in the past in attracting research scientists and engineers, then those funds have to be spent there at the expense of funds that could be used to maintain the quality of the MIT's and Stanford's, and that is what has produced the tension now.

As I say, there is no answer to this. It is firm and pervasive. But the Federal agencies are staffed up to handle this competition. There should be some funds—and there are—for both, but it is not something that I can make any simple answer to.

At the top of page 14, I talk about the fact that any expansion will mean that some others will need contraction. Contraction is difficult anywhere, but it is especially difficult at a university, and for good reasons. Contraction is hard at a national lab. I think it would be very difficult to decrease the number of Congressmen in Congress, and so on.

But universities have a particular problem with contraction because American universities, especially, flourish with decentralized management. Most of the management of universities is in fact decentralized to departments or to individuals, and under those circumstances, a contraction is particularly difficult. So it is not easy to ask some colleges and universities to contract in order that new players can enter the game on a level playing field.

Finally, I address your question three at the middle of page 14 and start with a disclaimer, namely that I am on less firm ground there, and about the only thing that I can contribute there is to say that when one comes to areas of generic research involving very closely the applications or potential applications of research, again this is a place, it seems to me, that the key to this is to let the individual institutions, States, and regions compete for Federal funds on the basis of their own aspirations and quality, and again, it seems to me that the agencies are in good position to run this competition.

Finally, on page 16, I point out, in response to your invitation to comment on congressional science relations, with a plea for more informal interaction between Congress and the scientific community. Your committee has done a good deal of this, and so I am not in any way criticizing your committee, but only to point out that it could become a better standard for the rest of Congress.

I realize the committees are overworked, having to go through the authorization and appropriation action every year. They have very little time for the kind of activity that your task force is now doing. But the informal interaction of your seminars—and I think some of you will be participating in the Government-University-Industry Roundtable sessions on July 22 and 23—this kind of thing, I think, is just absolutely essential, because as science gets even more ramified and complicated, without that kind of informal interaction with your staffs and scientists, it seems to me that you will be trying to get your arms around something which just will refuse to be encased in your broad arms.

Well, with that introduction, I am prepared to take your questions.

[The prepared statement of Dr. Sproull follows:]

U.S. HOUSE OF REPRESENTATIVES
SCIENCE AND TECHNOLOGY COMMITTEE
TASK FORCE ON SCIENCE POLICY

Testimony by Robert L. Sproull
on
SCIENCE IN THE POLITICAL PROCESS
June 25, 1985

Mr. Chairman and members of the Task Force. My name is Robert Sproull. I am appearing before you today as an individual, drawing on my experiences within the university and the Federal Government and as a member of the Council of the Government-University-Industry Research Roundtable, sponsored by the National Academies of Sciences and Engineering. I appreciate the opportunity to discuss with you the role of science in the political process.

I commend the Task Force for the questions it has posed on page 56 of the Agenda for its Science Policy Study to guide the discussion of this topic. They raise issues of critical importance to the nation. The Roundtable Council also has been studying these issues.

My remarks are organized around the three questions. I will address each in the order listed.

## 1. How Can the Expert Judgments of the Scientists and the Societal Goals-Oriented Judgments of Members of Congress Effectively Interact?

I warmly agree with the statement by the Task Force "that in the last 40 years the expert judgments of scientists and the broader political judgments of members of Congress have, for the most part, interacted successfully." Indeed, our system for the participation of both communities in decisions about science is the envy of the world.

Admittedly, there have been tensions and disagreements, but they have been relatively rare, especially in those areas that constitute the backbone of our scientific and technological research enterprise—support for research projects and programs carried out by individual investigators and small groups. Where difficulties have arisen, they have usually been related to "big science" projects and to cases in which local, state, and regional aspirations have been involved. It is instructive, therefore, to review the range of procedures that have been used in the past for allocating funds for research facilities.

Where the largest-scale facilities are involved, many considerations enter the picture. In the case of the National Accelerator Laboratory (Fermilab), more than 100 proposals were received by the Atomic Energy Commission. The Commission screened the sites to insure that they met the basic requirements, including adequate power and water supplies, adequate land areas and enough housing and transportation capacity. Once these determinations had been made, a committee of experts assembled by the National Academy of Sciences further reduced the number of proposals to those that met all the requirements for a successful national laboratory. This refined list was relatively short, and any site on the list was technically satisfactory. The Commission made the final selection, taking into account the interests of the Congress and relevant social, economic, and political priorities.

An example on a smaller scale was the National Science Foundation's Science Development Program--the "New Centers of Excellence"--in the 1960's. The goals of this effort, as documented in legislative history, were the development of new centers of excellence and the general improvement in the quality of science and engineering education. Wider geographic distribution was a primary objective. The Foundation established an advisory committee of scientific experts that studied every proposal. On the basis of the quality of the proposals, on the reports of independent site visiting teams and on the basis of criteria established by the Foundation, the advisory committee made its recommendations.

A major consideration in the final recommendations involved judgments about the likely gain in scientific productivity per million dollars invested. Other considerations involved the commitment of state governments to provide matching grants to their competing universities and the capacity of the proposing universities to recruit and retain faculty competent to conduct the new programs.

Discussion between the committee and the Foundation staff was an important factor in the deliberations. In the end, the funding decisions by the Foundation were designed to serve the ends intended by Congress in authorizing the program and appropriating the funds.

In some cases agencies have relied on their own technical staffs to provide the necessary evaluations. The case of the NASA Sustaining University Program of the 1960s, one-fifth of which went for facilities, is an example. Competing proposals were reviewed internally and decisions made by the Administrator on the basis of anticipated return on investment. The extent of an institution's involvement in NASA research was a major criterion.

A recent example is DOD's siting of its Software Engineering Institute. The impetus for the Institute came from the DOD in its FY 1985 budget proposal, and the line item funding provided by Congress was free of directive constraints. DOD issued a Request for Proposals that included criteria for evaluation relevant to the success of such an institute.

Proposals were reviewed first by an evaluation board including civilian and military DOD personnel as well as NASA and National Security Agency representatives. This board was selected for its technical competence, and it limited its evaluation to the technical merit of the proposals. Site visits were included. The next level of review was conducted by senior Defense executives—civilian and military. The final decision, to award the Institute to Carnegie-Mellon University, was made by the Undersecretary for Research and Engineering. While substantial lobbying was acknowledged, DOD asserted that the final decision was made on the technical merits of the winning proposal.

Political influence has been a factor in some cases, but its extent and pervasiveness are debatable. The emergence of the University of Washington as a center of excellence in biomedical research and medical education is often cited as an example of the salutary influence of Washington Senator Warren Magnuson, for many years Chairman of the Appropriations Subcommittee with jurisdiction over the National Institutes of Health (NIH) and later Chairman of the full Appropriations Committee. There are divergent interpretations of what happened during the period of rapid expansion of the University of Washington Medical Center.

One interpretation holds that Senator Magnuson was a powerful chairman who was generous to the NIH, and that the NIH was politically astute enough to recognize the advantage in supporting the University of Washington.

Another view holds that the growth of the Medical Center was apolitical—the absence of line item appropriations and the submission of Washington proposals to expert review are used to substantiate this view. This version holds that Senator Magnuson only assured, through his generosity to NIH, that there were ample funds to be applied for—by Washington and by all other qualified institutions.

From these examples, I conclude that the dominant decision-making process for allocating funds for research facilities has been one of comprehensive merit evaluation—a process based on a case-by-case examination of the technical merit, local capabilities and aspirations and other factors that impinge on the ultimate success of each individual facility proposal in meeting program objectives established by Congress and the agencies. The factors involved go beyond the review and judgment of scientific and engineering "peers" to include perspectives of those charged with exercising social, economic, and political judgments. I suggest some guidelines for carrying out such a process in the future in my response to question 2.

## 2. At What Levels Should Decisions Be Made by Scientists, by Members of Congress, and Jointly?

As illustrated by the examples above, while advice is provided by scientists outside the Federal Government, decision-making is reserved to federal agencies. Scientific evaluation of the promise of individual research and facility proposals is essential. However, decisions should be made by the executive agency responsible for managing the program, with interactions with the relevant congressional committees, after scientific and technical advice has been received according to each agency's pattern of doing business. Broader economic, social, and political considerations have been and often should be factored into the decision-making process by the agency and the Congress. The role of the various parties in decision-making about science is best illustrated by considering individual research programs and facilities separately.

Individual Research Programs. While the system for reviewing program proposals and allocating limited resources needs constant monitoring and will require adjustments from time to time, as stated earlier, it has been remarkably successful and its essential features should be maintained.

The accepted process, which has extended now over three decades, includes the following features.

The Congress, through its appropriation and authorization committees, approves R&D programs, establishes new programs, sets funding levels, and provides guidelines and objectives for the conduct of the programs. Executive agency proposals and advice from non-government scientists and engineers as well as others are used by the Congress in carrying out these functions.

The Executive Branch agencies, in consultation with the Congress and the scientific community, are responsible for designing mechanisms for administering the broad programs established by Congress; the agencies are also responsible for deciding on which specific R&D projects and programs to support. Research proposals are reviewed through agency established mechanisms designed to select, from among all those submitted, the proposals that are judged to be of the highest technical quality and, thus, that best promote high quality science and engineering. Ordinarily, scientists and engineers who are acknowledged experts in the fields at hand have been chosen to make the reviews.

Systems for the review of individual research proposals vary from agency to agency. Procedures sometimes include assembled groups of reviewers, and in other cases proposals are referred to selected individual reviewers. In some cases the review is conducted by technical staff within the agency. The NIH use assembled groups in a two-tier process in which the first level of review is scientific and technical, and the second considers the awarding unit's mission, along with other relevant factors.

The central feature, however, of all such reviews is the use of recognized experts who are qualified to judge the significance and the relative quality of the proposals at issue. Within the proposals judged to be of high quality, agency staff normally exercises some discretion, based on available funds as well as on other criteria, in deciding which proposals to fund. The factors considered by the staff vary from agency to agency and from field to field, but include relevance to program objectives, as well as other concerns of a social and political nature.

In exercising this discretion, agency staff should be responsive to the congressional intent in authorizing and funding the program. If Congress has stated explicitly that factors such as establishing new centers of research strength or stimulating local economic development are objectives of the program along with promoting scientific and engineering excellence, then the agency is responsible for supporting projects consistent with these objectives. Even without such a specific congressional mandate, the agencies and the Congress interact throughout the year, formally through hearings and informally in many ways during which special aspects of R&D programs can be discussed.

Within the system for allocating resources for individual research programs, the roles of scientists and engineers, of agency managers, and of the Congress seem to me to be totally appropriate. All three communities have worked conscientiously and effectively to create and operate a most successful enterprise.

Facilities. Support for research facilities has been an arena of rapidly increasing tension. Why?

1. Research facilities established at universities during the 25 years following World War II have deteriorated and are obsolete.

2. These facilities were established during periods following the War (immediate post-War; post-Sputnik; and the "great society" of the 1960s) in which federal and state government programs encouraged the universities to expand their research and teaching capacities.

3. Today, the enterprise is no longer expanding. The 18-year old cohort is not expanding. Federal funds for research, in real terms, have not been increasing consistently over the past decade and they are not likely to over the coming decade. National investment in research facilities has declined markedly; there are almost no categorical federal programs providing support for facilities. The two percent use charge for facilities allowed in federal research grants and contracts to universities is totally inadequate to replace obsolete facilities or to do major renovations to place modern research facilities into an old shell. Even if the shell lasts forever, major renovations costing as much as the shell must be done every 15-20 years.

4. Maintaining research and teaching programs at the frontiers of science and engineering, meeting health and safety standards, and following best laboratory practices require modernized facilities.

5. The communities and geographic regions surrounding universities view academic research facilities as important components of their economic development strategies.

6. To meet facility needs, university administrators and scientists, sometimes accompanied by state and local officials, seek to raise funds from all available sources, including in some cases the unusual route of going directly to the Congress.

7. Members of Congress are willing to help, because they were elected to promote the economic and social development of their districts as well as to serve the national interest.

I am not in this testimony recommending any specific federal or other type of program for facilities. On July 22-23, the National Academies, the National Science Board, and the White House Office of Science and Technology Policy are sponsoring a working conference intended to identify and examine strategies for meeting facility needs. The academic facilities legislation you, Mr. Chairman, recently introduced, will certainly be one of the strategies examined. Congressional participation is planned, and the results of the conference will be shared with the Task Force.

I will address my remarks to the principles that should be followed in carving out roles for the scientists, the federal agencies and the Congress, as you consider means of direct federal support for facilities.

1. The role of Congress in direct federal support of facilities is the authorization and funding of programs. By definition, this

includes establishment of program objectives, and criteria, if any, in addition to scientific and technological merit and promise.

2. Procedures involving direct federal funding for facilities, for example, grants, loans, interest payments, and use charges, should be managed by the federal R&D agencies on the basis of comprehensive merit evaluation of discrete proposals. (Indirect approaches also are possible such as realistic depreciation rates in research grants, payments for rent in research grants, and formula funding, but such mechanisms do not lend themselves to facility proposal review.)

   The technical features of such evaluation should be designed to ensure that:

   a. The programs of the institution in question are adequate to achieve the stated goals;
   b. The people conducting the programs proposed for the facility in question are capable of competent execution of the programs;
   c. The proposed institution is the institution best able to achieve the goals intended by Congress and the agency;
   d. The capacity of the area, or of the institution, is adequate to provide the transportation, communication, supplies, water, and other necessary services; and
   e. The cost of the facility will be reasonable.

   Sometimes the ability of the institution to meet these criteria depends on commitments by other bodies, such as the willingness of a state to fund new positions if federal support for construction of the facility is assured.

Generally, those called on to make these technical judgments should be selected, by procedures established by the federal agencies, from among recognized experts in the fields in question.

Other factors of a more social, political, and economic nature also are, and usually quite appropriately, taken into account. For example:

a. Geographic distribution—the contribution to building research and educational capacity in certain institutions and regions of the country;

b. The contribution to local and regional economic development, both through the direct impact of jobs to build and staff the facility, and the indirect impact of spin-off companies and the attraction of additional technological enterprises to the area.

c. The willingness of the institution, of the state, or of the region to share costs and to otherwise express its determination to invest its own funds in the facility and its programs.

It is appropriate for Congress to insist that individuals capable of exercising these judgments and in whom it has confidence—not necessarily those with expertise in the scientific and engineering areas—be involved in the decision-making process.

In sum, technical review by experts in the relevant fields is necessary to limit the choices to those that meet the technical criteria; other factors may quite properly be considered in selecting among these.

3. Federal approaches to facility support should be based on cost-sharing among the Federal Government, state governments, universities, and the private sector. This allows institutions, states, and regions to demonstrate their initiative, commitment, and confidence in seeing the facility develop into a successful enterprise. For example, the Federal Government might offer low interest loans for facilities which the states and the institutions would agree to pay back on the assumption that the facility will attract continuing support for its R&D programs and that realistic depreciation or use charges are allowed in the R&D grants and contracts. Local commitment is an important criterion in the comprehensive merit evaluation process.

This local commitment factor in the evaluation process is especially important in the current context for support for R&D and research facilities. As I stated earlier, several indicators can be cited to illustrate that the R&D enterprise is not expanding. Under such circumstances, any new start will have an impact on the existing enterprise. It is in the national interest to maintain the quality of this enterprise. This judgment is susceptible to the criticism, "To him that has shall be given." This is a serious concern to which I respond as follows:

(1) Current centers of strength in academic science and engineering are the result of long-term investments by universities, states, private donors, and the Federal Government; it is in the national interest, and it is an effective use of resources, to build on this strength; (2) At the same time, by including local commitment and investments as a component of the evaluation process for facility proposals, any institution, state, or region with the ambition to establish a new or expanded R&D capability has a way to compete effectively for federal funds to help it do so; and (3) The overall size and scope of the educational and research enterprise requires examination in the light of current economic, demographic, and scientific factors; some parts of the system will need expansion and others will need contraction.

3. Under What Circumstances Should the Congress and/or the Scientific Community Use Criteria such as Regional Economic Growth, Specific Health Needs, and Agricultural Crop Needs in Making Decisions for Science Policy?

I feel on less firm ground responding to this question than to the others and therefore will make only a few brief points.

First, as indicated by my comments above, the scientific community has no special usefulness in applying criteria such as economic growth to decisions about science. The agencies and the Congress, when appropriate and based on advice from relevant individuals, are the organizations that must bring such criteria into the science policy process.

Second, criteria such as specific health needs and agricultural crop needs are critically important in establishing priorities for generic research (the gray area between basic research and applied research) and applied R&D in the agriculture and biomedical areas. I cite here the findings of a recent study of the relationships between federal R&D policy and technological change in seven major American industries—semiconductors, computers, aircraft, pharmaceuticals, agriculture, residential construction, and automobiles.[1] The authors found three types of policy that have been successful in the past: 1) government R&D support for technologies in which the government has a strong and direct procurement interest; 2) decentralized systems of government-supported research in the generic areas between the basic and applied; and 3) a decentralized system of clientele-oriented support for applied R&D.

Features that were found to be keys to success in areas 2) and 3), in which the agriculture and pharmaceutical fields are included, are: a) Involvement of both the scientific community and those with applied interests in establishing R&D priorities; and b) Evolution of the research programs, and of the institutional structures for sponsoring and performing the research, on the basis of the needs and desires of the scientific community and of those concerned with applications; the initiative and the design of the programs were not centrally orchestrated.

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1/ "Industrial Innovation Policy: Lessons from American History" Nelson and Langlois, Science, 219, 18 February 1983, p. 814.

In light of the above findings on decentralized support for generic and applied research, we need to think of the states as an important locus for policy leadership. They are demonstrating the ability to stimulate linkages between knowledge-producers and knowledge-users and to invest substantially, sometimes with federal stimulus, in areas of technology identified by these alliances as having the greatest promise. It is also noteworthy, that the state programs are involving both the public and the private universities.

## Congressional - Science Relations

My remarks have focused on the interactions among the Congress, the scientific and engineering community, and the agencies in the formal process of program formulation, authorization, and execution. I conclude with a plea for more informal interaction between the Congress and the scientific community. The activities of the House Science and Technology Committee, Mr. Chairman, set the standard in this regard. The Science Policy Study of your Task Force, which brings us here today, and the science seminars held by your Committee over the years, are effective means for bringing the Congress and the technical community together outside of the legislative hearing process to examine major science policy issues and to review scientific and engineering advances and their implications. I would hope that other committees could be encouraged to do likewise. The scientific and engineering community stands ready to help create opportunities for more informal interaction, as indicated by the congressional seminar programs conducted by several of the professional societies.

## DISCUSSION

Mr. **Fuqua**. Thank you very much for a very thoughtful presentation.

I am not sure of the total accuracy of the figures —they are in the general ballpark—but it has been pointed out that if you take the top 50 institutions in the United States, and if you add up all the Federal research funds that they get, that they get approximately half of all the Federal support. The remainder of the schools fight for the other 50 percent. That was true 20 years ago, and even though funding has increased over the time, that is still the case today.

Now, these that are below there, from 51 through whatever number it is that participate in Federal funding programs, tend to say, well, you know, we have to look outside the peer review process because it hasn't helped us any. That same distribution is prevalent today. How do you respond to that?

Dr. **Sproull**. Well, I respond by saying I couldn't disagree more violently. There are going to be a different 50 institutions in the next couple of decades. There already are a lot of changes. The American Association of Universities—I think they just added a few. Newton Cattell is around here someplace. Is it about 54 or something like that now?

Mr. **Cattell**. I think it is 54.

Dr. **Sproull**. But some of those institutions don't belong in the AAU; they have really ceased to be front-ranked universities in a research sense. And there are lots of others that are very ambitious that should be in if that is to be a club of the research universities. So there is a lot of jockeying in and out of that group of 50, and quite appropriately.

The second thing I would say is that those institutions, even small colleges—for example, you have a first-rate college like Swarthmore that wants to have a certain amount of advanced work, but it wants to keep it in check. It doesn't want to become a research university, but it wants to have some advanced work. It gets a good deal of Federal funds. It doesn't get as much as it wants, but I think it gets as big a fraction of what it wants as, say, Cornell University, where I spent most of my life, which is certainly one of the "haves."

I belong to the University of Rochester, which is kind of intermediate. It is a small university, but it has lots of Federal funds.

So, first of all, there is some jockeying. Secondly, there is an opportunity for the small colleges. Third, there is an opportunity for the newer regions, particularly if the region is willing to say, "Look, we are ambitious to be the new MIT of the next century; we'll put this, this, this, and this with it, and then become an effective competitor for funds."

That is exactly what Stanford did in the thirties. Stanford was a nothing university until Frederick Tiernan had the ambition, in the 1930's, to make it into a modern research university. He took lots of local ambition and made it transfer from rhetoric to funding.

They got no particular benefit from World War II. Their people were away at MIT, at Los Alamos and other places. They got no particular Federal benefit from that. It was local determination given effect to by committing their funds and their time that produced the Stanford of today, and this process goes on.

Finally, let me say about the "old boy network" and research funding, that I served, when I was a bench scientist—I don't any more; nobody trusts my judgment as a scientist, quite properly—but I served on a great many of these committees. I was an advisor to ONR almost from the beginning of ONR in 1946.

I found that, over and over again, the young investigator, the new institution, is like a breath of spring in those meetings. If anything, the committees and the peer review leaned over backward favoring the new, the fresh, the promising but unproven.

I think, also, I have seen some abuses of that. I have seen some aggressive individuals who will not be named since this is on the public record, who make such asses of themselves in those meetings that the whole scientific community knows about it within a nanosecond afterward. [Laughter.]

So the peer review system is imperfect, like any human system, but it has worked very well, and I don't think the image that it has of a closed system with people giving money to their friends is at all appropriate. If anything, we have leaned over backward in the opposite direction.

I don't know; it is a long-winded answer, but——

Mr. FUQUA. It is very important.

Hardly a year goes by, when we consider, say, the NSF budget that we don't have complaints from members that they feel like there is an inadequate geographical distribution; that States or universities located within States, certain States, are still not getting what they perceive as a fair shake in the research funds that are distributed.

We are all, in the Congress—in the House, 435—from various parts of the country. The pressure has been on to do something about that. We, even in the NSF bill, provided for a modest program to try to help some of those schools get better prepared so that they could compete more on an equal footing, but that is only a modest amount.

But that is still a process. I hear this on the floor. We bring the bill up. Members come up and say, I'm going to vote for this, but I'm very disappointed that there hasn't been a better—they are not advocating a formula, that it be based on a formula basis, but that there is not enough effort made to get a better geographic distribution.

Of course, we live in a political system. I would hate to see a formula-type thing come forth. But there is a lot of pressure to do that. Yet we want to get the best science that we can get, if the Government is involved in it, and it should be on a meritorious basis. How do you equate those pressures into reality?

Dr. SPROULL. With great difficulty, and I am not sure I have anything to say that is worth your time to listen to.

But I believe that Government is for the purpose of doing things for the citizens that the citizens cannot do for themselves, and that the Federal Government is for the purpose of doing things that the States cannot do for themselves.

Support of the scientific establishment, deciding how much of the taxpayers' money should go into high energy physics versus solid state physics, seem to me to be a bona fide Federal responsibility and particularly, ultimately, a responsibility of the Congress, with getting the best advice it can get and the best analytical service from the agencies.

But, on the other hand, when it comes to development of particular areas of the country, every area of the country is ambitious. It seems to me that this is, first and foremost, a State prerogative. The State of Florida, where I live a good deal of the year, has its ambitions. The State of New York—sometimes I am unsatisfied that it doesn't have higher ambitions.

Those two should, it seems to me, put their money where there aspirations are; for, when it comes to economic development and the development of a population of trained people that go with economic development, this is primarily a local and institution and State and perhaps consortium of States and regions—such as the Oak Ridge Associated Universities—prerogative.

So I guess the only wisdom—and it probably isn't very wise—that I would like to insert here is that Congress might very well take the high ground and say, "Let us deal with those things that States cannot do for themselves, and not participate strongly in the competition from one State to the other based on economic aspirations."

I am not sure that helps.

Mr. **Fuqua**. It is about as good an answer as I have come up with, too.

Mr. Brown.

Mr. **Brown**. I pass, Mr. Chairman.

Mr. **Fuqua**. Mr. Lundine.

Mr. **Lundine**. I was interested in your discussion on page 15 about the relationship between Federal R&D policy and technological change. While I understand and agree with the conclusions of the study you cited, isn't it also necessary for those of us with oversight responsibility to be concerned about other trends; for example, the amount of research and development activity going into military versus civilian purposes?

I mean, is that a valid concern right now, in spite of the fact that, obviously, the Government will support those technologies in which we have procurement interests?

Dr. **Sproull**. Of course, it is. And it is, first and foremost, it seems to me, a concern of Congress.

One of the well known and important distinctions between us and Japan is that whereas the United States provides the security umbrella over Japan, they can put a much larger fraction of their gross national product into civilian-oriented research and development, and it probably is one of the two or three major elements in the result that we are not competing all that successfully against Japan.

So certainly that is a concern, and it is a concern foremost of Congress. The economic development of the country as a whole, it seems to me, is a concern.

Let me go back. I know the committee is interested in whether the situation we have now of tensions over congressional participation in individual projects is an example, new, or whether we have been there before.

If you go back to the Sputnik days, Congress, in the fall of 1957 and spring of 1958, interacting with the Federal agencies, made a very strong determination that the response to Sputnik had to be economic as well as military in space, and in fact a great deal of work was supported in that period on the infrastructure which has made the computing and information technology/communication industry in America the preeminent industry in the world. A lot of our survival has been because computing/information technology/communication still is a place where we can export, still is a place where even New York State has a great deal of viability and export ability.

So that seems to me to be a valid concern, and it is a concern that Congress has, over the years from time to time, taken a very leading role in.

I don't think it is appropriate to ask the scientists to decide how you balance Federal support for research and development that underlies industry competing in world markets versus the science and technology that underlies national security. I don't think that is a role of the scientist at all. I think that is the role of Congress, as advised by the agencies.

Is this responsive to your question?

Mr. LUNDINE. Yes, it is.

I do have a related question, though. Do you think that there is a serious problem of human infrastructure now? You spent a fair amount of your talk about the equipment and the obsolescence problem and all that. Is that a basic problem because we are not attracting enough people, or am I making a connection that you didn't intend?

Dr. SPROULL. No, no, you are right. I made the case, and I should be a little more specific bout it. Let's take the case of chemical engineering, for example. Chemical engineering is an absolutely basic element of a technological society, and it is one of the elements that has traditionally been strongest in America and West Germany and the United Kingdom, I guess.

We have been accused in universities of not attending to the public interest because we have so many foreign graduate students in a field like chemical engineering. The fact is that because of our antiquated facilities, because of our necessity for sharing costs with the Federal Government on research funds, we have been having a very hard time attracting American students away from industry to do advanced work in chemical engineering. It is almost a national disgrace, and all of us should take some of the blame for it.

But the facilities are only part of it. But the replenishment of a new generation of chemical engineers with advanced training is something that the Congress, the agencies, the universities all have to take some of the blame for. We are just perilously close to disaster in areas like that.

Computer science is another area of great shortage of the human infrastructure, but one that we should not blame ourselves so much for because it is because of the huge expansion of ideas and promise in that field. The field opened up so fast and with such spectacularly interesting things going on that I don't think any of us could have prepared for that in such a way as to avoid shortages.

But surely the human infrastructure is more important than the facilities, but the facilities are a part of the reason we do not have the human infrastructure.

Mr. LUNDINE. Thank you, Mr. Chairman.

Mr. FUQUA. Dr. Sproull, we want to thank you for being with us this morning and sharing your perspective. You certainly come from a varied background that is very helpful to us, and we are grateful to you for sharing your time and wisdom with us this morning. I wish we had some simple answers.

[Answers to questions asked of Dr. Sproull follows:]

## QUESTIONS AND ANSWERS FOR THE RECORD

Dr. Robert L. Sproull

1. **The recent cases of attempts to obtain science facilities for individual universities through the political process are not the only instances where conflict between political and scientific judgments have taken place. We have also seen, for example, attempts by the Congress to establish new, mission-oriented institutes at the National Instututes of Health (arthritis, nursing) which the medical research community opposed. Do you see any generally applicable lessons that can be drawn from all of these cases?**

   It is not possible to make a clear cut, definitive statement about these cases, but there is a guideline that I should like to suggest: Insofar as possible Congress should decide what should be done and leave to the Executive Branch exactly how to do it. In the NIH case of arthritis and nursing, Congress might well decide that more national attention should be paid to these fields and more federal funds should be spent on them, but leave to the Executive Branch the decisions as to whether to create new institutes in the NIH or to expand programs within existing institutes. Similarly, in the instant case of funding science and technology facilities in colleges and universities, Congress might very well set up a program and criteria for selection but leave selection of the recipients to competitions managed by Executive agencies. HR2823 does this.

2. **Those who have expressed a concern about efforts to obtain research facilities by a resort to the political process have noted that some projects have been initiated not only without scientific review, but also in the case of floor amendments, without review in the Congressional system; that is, through subcommittee and committee hearing and debate. Would legislative provisions for such facilities be more acceptable if they were the result of such a more extensive Congressional review?**

   Yes. The most powerful and compelling objection to the "end-run" appropriations (such as Columbia and Catholic Universities) is not that they were executed exclusively by Congress but that there was no competition, no weighing of such projects against other uses of like funds in the public interest. If each project is looked at in isolation, perhaps in floor debate without hearings or committee review, it is tiny with respect to the federal budget, and the answer is likely to be: "Why not? It will be a help to the institution and the region and no one will notice its effect in the total federal expenditures." This is grossly unfair and wasteful. There may be scores of other uses of those same funds that would enhance the public interest more. If Congress could exercise the self-restraint of voting facility funding for colleges and universities only pursuant to competition within an announced program and to committee analysis and ranking, the damage to the educational establishment would be reduced; if in addition, committee analysis could always include the scientific merit of the projects (along with other considerations), the Congressional facilities actions would be wholly constructive and uniformly applauded. Of course this would place an enormous burden on Congressional committees which would have to duplicate the staffs of existing Executive agencies; the more efficient approach would be for Congress to create and fund the program, (including setting criteria) and charge the Executive Branch with administering it.

3. **In the event we continue to see the practice of lobbying for research facilities or other attempts to make judgments about science within the political process at about the level of the past 3 to 4 years, what, if any, will the long-term effects be, in your opinion? Is there a threshold level where this practice has serious adverse effects, in your view?**

The effects, in my view, will be devastating. Already the Congress has made it much more difficult for us to raise money for facilities through our own boards of trustees (private institutions) or from our legislatures (state institutions). Raising money is hard work, and a trustee knows that he must contribute generously himself before he can effectively solicit contributions of others. The Congressional disease (use of lobbyists, Floor amendments) has the complications that trustees now say: "Why don't you be smart like the presidents of these 'end-run' universities? Don't come to us for private contributions, hire a lobbyist." Merit, promise, economic development and other considerations will be replaced by the law of the jungle. As to the second question, that threshold had already been passed; one or two highly publicized instances a year are more than enough to create serious adverse effects.

4. **It is being argued that the Congress should provide general goals and guidelines for science programs, including science facility programs, and let the agencies of the Executive Branch do the actual selection of the institutions and individuals to receive support. Yet in other areas where the Congress provides support, most notably in the defense procurement and the defense construction areas where a high degree of expertise presumably also is needed, the legislative provisions are highly detailed ("$18 million for M-1 heavy trucks", "735,000 for a refrigeration building at Tyndal Air Force Base"). Is there any evidence that one system is notably better or worse, or are the different approaches mostly a matter of tradition and practice?**

   I find it impossible to suggest any doctrine or sharp distinctions here. There is and must be close interaction between Congressional committees and Defense departments and agencies on all such programs. Because of their sizes, many of these deserve and get close Congessional scrutiny and line-item attention in the legislation or in committee reports. I believe this is wholly appropriate for large programs like weapon systems or a new research program like the Strategic Defense Initiative. The fact that I think it is quite inappropriate for an item like "$735,000 for a refrigeration building at Tyndel Air Force Base" is probably of no consequence; I am sure the Congress will not soon give up its oversight of such items of "MilCon." But it is bad policy and leads to gross inefficiency; I see no reason to extend bad policy to science and technology facilities for universities.

5. **Apart from the question of the respective roles of the members of the political world and of the scientific community, what means do we as legislators and you as members of the scientific community have to "enforce" or at least encourage compliance with whatever policies or guidelines that we may arrive at in this area?**

   Congress will have to decide whether it can discipline itself; far be it from me to comment on that! As to the "members of the scientific community," there is no way of enforcing self-restraint or even morality on them. They are too diverse and fiercely competitive, both valuable attributes. What can be done is to provide incentives (the "encouragement" referred to in the question) to work through established programs. The recent Congressional actions have provided exactly the wrong incentives. If the Congress were to take some responsibility for the health of the science and technology establishment of the Nation (including its facilities) and create appropriate programs, incentives in the proper direction would accomplish the intent of this question.

Mr. **Fuqua.** The task force stands adjourned until tomorrow morning at 10 o'clock in this room.

[Whereupon, at 11:37 a.m., the task force recessed, to reconvene at 10 a.m., the following day, Wednesday, June 26, 1985.]

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# SCIENCE IN THE POLITICAL PROCESS

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## WEDNESDAY, JUNE 26, 1985

House of Representatives,
Committee on Science and Technology,
Task Force on Science Policy,
*Washington, DC.*

The task force met, pursuant to recess, at 10:04 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Fuqua. The Task Force on Science Policy will be in order. Today we continue in the second day of hearings on "Science in the Political Process." We had two very excellent witnesses yesterday, and today, I think, equally, we have excellent witnesses that can give us their perspectives on the role of the political process in science.

Our first witness today is John R. Silber, who is the president of Boston University. Mr. President, we are very pleased to have you with us today and will be delighted to hear from you.

[The biographical sketch of Dr. Silber follows:]

### Dr. John R. Silber

John R. Silber has been President of Boston University since 1971. President Silber was educated at Trinity University in San Antonio, graduating summa cum laude, and at Yale University, where he earned his Ph.D. in Philosophy. He is an authority on the philosophy of Immanuel Kant and the philosophy of law. Between 1955 and 1970, Dr. Silber served as Professor of Philosophy, Chairman of the Department of Philosophy, and then as Dean of the College of Arts and Sciences at the University of Texas at Austin. As President of Boston University, he has successfully improved the quality of the faculty and student body and, under his leadership, has achieved unprecedented financial stability. He is a leading national spokesman on educational and social issues. In July 1983, President Reagan appointed him to the National Bipartisan Commission on Central America.

## STATEMENT OF DR. JOHN R. SILBER, PRESIDENT, BOSTON UNIVERSITY, BOSTON, MA

Dr. Silber. Thank you, Mr. Chairman.

Chairman Fuqua and members of the Science Policy Task Force, I am very pleased to have this opportunity of discussing the place of science in the political process. Not only is this an issue with important implications for Federal policy regarding science and higher education; it is also an important issue from the perspective of political philosophy.

The role of science and scientists in politics is a specific instance of a far wider question that has been debated by philosophers since at least the fifth century B.C. Broadly stated, the issue is whether we should be ruled by experts.

Plato, in the Republic, sketched out an elaborate educational scheme designed to maximize the potential of each member of society. But only the most successful recipients of this education would acquire the full knowledge and virtue which, in his view, were required of rulers.

The remainder, those without the natural ability to enter the class of rulers, would fall naturally into other categories and in these roles would make important and even essential contributions to their society. But they would be properly subordinated to those qualified by talent and education to know what was best for everyone else.

Many philosophers since Plato have been drawn to the vision of a fully just society as one in which every individual is given the opportunity—an equal opportunity uninfluenced by wealth or family circumstance—to reveal his inclinations and to develop his capacities through a system of universal education.

Indeed, it was this aspect of Plato's thought that made a major impact on Thomas Jefferson and other Founding Fathers, who held that each individual has an inalienable right to life, liberty, and the pursuit of happiness. From Plato came the firm belief, shared by Jefferson and Adams, that these rights can be achieved only if education is available to each individual.

But most philosophers have been reticent in proposing schemes to rebuild society in a way that forces everyone to participate in a comprehensive system for education and social classification, for they rightly recognized that this would entail the creation of a totalitarian state and thereby destroy that very core of individual freedom on which equality of opportunity depends. The concern to preserve human freedom was, of course, central to the work of the framers of our Constitution.

Perhaps the most devastating critique of Plato's apotheosis of the expert can be found in the work of his younger contemporary, Aristotle. In *The Politics,* Aristotle observes:

> If the people are not utterly degraded, although individually they may be worse judges than those who have special knowledge—as a body, as a group, they are as good or better.
>
> Moreover, there are some arts whose products are not judged solely or best by the artists themselves; namely, those arts whose products are recognized even by those who do not possess the art; for example, the knowledge of the house is not limited to the builder only; the user * * * of the house will be even a better judge than the builder, just as the pilot will judge better of a rudder than the carpenter, and the guest will judge better of a feast than the cook.

Aristotle here succinctly expresses a fundamental principle of American democracy, that in matters affecting the society as a whole, those whose lives are affected—namely, the people—are likely to be a better judge than any class of experts.

It is for this reason that the Constitution reserves to the Congress, to the elected representatives of the people, not only the power to declare war but also the power to levy taxes and to judge the purposes for which tax money shall be used.

Just as in providing for the common defense, the people through their elected representatives shall decide what they shall be defended against; so also, in promoting the general welfare, the people shall determine through their elected representatives what is truly in their best interests.

This does not mean that the opinions of experts have no value or that they should be disregarded entirely. It does mean, however, that the Congress cannot delegate to experts its ultimate responsibility for promoting the general welfare. Legislators who must decide on issues that can be illuminated by experts have an obligation to seek out and listen to those who are best qualified to advise them.

But under our system of government, the only competent experts on what constitutes the common good are the people themselves, acting through their elected representatives. This, in my opinion, is exactly as it should be.

Being the best cardiac surgeon in the world makes one an expert on diseases of the heart and on some ways to cure such diseases. But no surgeon, however skillful, is qualified simply by his professional skill to pronounce on whether society as a whole would be better off spending millions to perfect the artificial heart rather than spending that same amount of money to reduce the national deficit.

In fact, common sense suggests that our hypothetical surgeon, precisely because of his preoccupation with cardiology, may be less able to judge objectively among competing goods when one of those goods happens to be the very thing to which he has devoted his life. This is the psychological truth behind Aristotle's observation that, as judges of their own welfare, the people are at least as good as, and often better than, those who have special knowledge.

In this context, much of the confusion that has obscured the subject of science in the political process disappears. Do Federal investments in science involve merely technical scientific issues on which scientists are particularly expert? Obviously not.

Every thinking person knows that the massive Federal support for science and technology that began during World War II and that has continued up to the present reflects America's rise to world leadership and our national need to retain military and industrial primacy in the face of the military threat posed by the Soviet Union and the economic and industrial threat posed by Japan and other nations.

Over the past 40 years, the intellectual geography of American science has been transformed in response to national priorities. There were only a handful of elementary-particle physicists working in the United States before the Second World War. By the late 1970's, largely as a result of this field's importance for our national security, there were nearly 2,000.

A similar explosive growth occurred in space science in consequence of our commitment to the Space Program, a decision made largely on political and strategic grounds. Ninety-six percent of the Federal dollars spent to support the 19 federally funded research and development centers go to just 10 of the FFRDC's, and these are concentrated in the areas of weapons research, space research, high-energy particle physics, and plasma physics.

This point is made even clearer when we consider how Federal funds are distributed between applied and basic research. Of the $50.9 billion in federally sponsored research in fiscal year 1985, approximately $43.1 billion is dedicated to specific, mission-oriented applied research, while $7.8 billion is dedicated to basic research.

While one might quarrel with these proportions, it would be absurd to argue that they somehow reflect a natural distribution based only on expert judgment of scientists. Scientists left alone would probably spend a much higher percentage on basic research. But, rather, they reflect the judgment of the Congress based in large measure on considerations of national security and economic and industrial growth.

Yes, sir?

Mr. **Fuqua**. Mr. President, we have a vote, and I think it might be appropriate if we take a break at this point, and we will be right back. I apologize for the inconvenience, but it is one of the hazards of the trade around here.

Dr. **Silber**. It is all right; I understand.

[Recess.]

Mr. **Fuqua**. The task force will be in order. We will resume, Dr. Silber.

Dr. **Silber**. Thank you very much.

In light of my previous comment on how the funds were being distributed between applied and basic research, it seems to me we should therefore not allow ourselves to be misled by claims implying that Congress is somehow shattering a time-honored precedent if it invests public moneys in scientific research only after considering the full range of issues that affect the public interest, for this is in fact the way they have done it for the last 50 to 60 years.

The very structure of American science is itself the result of a long and generally consistent series of such investment decisions made by the Congress since the end of World War II. It must be emphasized again that, under our system of government, at least as far as I am concerned, this is exactly as it should be.

In recent months, however, certain sectors of the scientific community—including a quintessential special interest group, the trade association of the major research universities—have engaged in much public handwringing. The pretext for these cries of alarm is not a revolutionary change in congressional policy toward scientific research. Instead, it is claimed to arise from just 15 congressional actions, amounting to a total appropriation of approximately $100 million, to help 15 universities build new or improved facilities.

The National Science Board claims that all of these projects involve facilities for scientific research, but the claim is not accurate. Several of the projects they cite involved instructional or training facilities having nothing to do with scientific research; having something to do, in many cases, with the preparation of persons who will later be qualified for scientific research. All of the facilities in question are mixed-use facilities, not strictly research facilities, and many of them have very important economic implications.

Boston University, as most members of the task force already know from my testimony before the Committee on Science and Technology on May 8, 1984, was one of the universities that received direct congressional funding. We were granted $19 million in Federal funds to assist in the development of a new science and engineering center.

The total cost of this project is approximately $90 million, three-quarters of which will be financed by industry, by philanthropy, and by Boston University itself. I refer members of the task force to my earlier testimony for the details of this project as well as for additional views bearing on the subject of this morning's meeting.

It might be supposed that any objections to such projects from within the scientific and academic community would stem from doubts—perhaps even supported by some evidence—that the research performed in these new facilities would not measure up to the highest standards of scientific endeavor.

But that is not the case. Not one scintilla of evidence has ever been adduced to support such a view, and the authors of the recent article, "Peer Review and the National Interest," one a former director and the other a current senior employee of the National Science Foundation, explicitly say, and I quote: "The point at issue is not whether meritorious research will be carried out in the facilities" obtained by direct congressional appropriation.

Instead, virtually all—and certainly the most strident—of the objections have been raised against Federal appropriations for new science facilities, and they rest upon a far stranger argument: That by providing this minimal aid to renew the Nation's obsolete and obsolescent scientific infrastructure, the Congress is somehow undermining the system of peer review.

The argument is false on its face. In the first place, it completely ignores the fact that the funds to build the facilities in which scientists work have, for the most part, never been subject to the traditional peer review that is applied to proposals from individual investigators for basic research grants.

In addition, the new facilities built with Federal funds not only will not diminish the effectiveness of the peer review system but will actually increase its scope. We will provide facilities in which researchers can come and from which they will apply for grants under peer review.

We should first be clear about the limited extent of peer review in federally funded research. The peer review system does not, for example, apply to the 19 federally funded research and development centers, which are typically owned by the Federal Government and operated by host universities under contract arrangements with the sponsoring Federal agency.

Funds for these Federal laboratories—which have amounted to more than $2.5 billion in each of the past 3 years, appear as a series of line items in the Federal budget and are not subject to peer review.

Nor does the peer review system apply to most of the research funds distributed by the U.S. Department of Agriculture. Most USDA research funds are distributed to State land-grant colleges and universities by means of formulas that do not consider the academic quality of the research but rely rather on the relative size of the rural and farm populations in the States where these institutions are located.

In fiscal year 1985, the Department of Agriculture will fund nearly $940 million in research, approximately half of which will be conducted at universities. Only $48 million of that amount, or 5 percent, is subject to peer review.

In fact, the peer review system, which uses panels of outside evaluators to rank competing proposals for limited research funds on the basis of merit, operates mainly in the National Science Foundation and the National Institutes of Health. A somewhat similar system is also in place for nuclear and high-energy physics research funded by the Department of Energy.

This is, of course, just what one might expect. These agencies and programs are primarily designed to provide grants to individual investigators to support basic research. Their purpose and organization are better suited to the use of external advisory panels to comment on the applications from the university-based researchers than the mission-oriented agencies like NASA, the Department of Defense, or even the Department of Agriculture are.

Because Federal support of science and technology is spread over so many agencies, and because the mechanisms for providing that support are so various, it is difficult to determine precisely how much federally sponsored scientific research is actually subject to peer review.

A conservative estimate, however, would be that no more than $4 billion out of $50.9 billion—or less than 8 cents out of every Federal dollar spent on science and technology—falls under the peer review system. And almost all of the research support that is awarded through peer review comes to university faculty members and is carried out in laboratories and other facilities located on university campuses.

When Congress spends funds to establish additional centers or to upgrade the quality of existing centers, in the process it increases the number and quality of places in which work is done that appropriately falls under the peer review system. By increasing the university-based facilities in which high quality scientific research can be performed, Congress thereby increases the scope of the peer review system.

This, then, should be an occasion for rejoicing within the scientific community. Yet, as we have seen, a few scientists, as well as some members of the Federal scientific bureaucracy and some major research universities, are not rejoicing, and it is very important that we examine the reason why not.

To understand this apparently mysterious phenomenon, it is necessary to pierce the veil of self-serving rhetoric that surrounds the peer review system. No one would deny that the principle of peer review is commendable. It is particularly commendable when it is applied in the appropriate way to the appropriate programs. On the other hand, no one should pretend that it is not a human institution and therefore not also fraught with inadequacies.

In practice, however, the system of peer review now in operation in this country is seriously flawed in ways that are not necessary and not intended by the idea of the practice itself. Approximately, 20 institutions, clustered in only three geographical regions, receive nearly half of all Federal research support.

In fiscal year 1983, for example, 20 institutions received approximately 40 percent of the total awarded to all 592 research universities receiving Federal research and development support. In fiscal year 1983, the top 10 institutions receiving support from the NSF received 30 percent of all NSF funds, and the top 20 institutions received 46 percent of all NSF funds. The top 20 recipients of NIH support have been the same institutions for the past 10 years.

The peer review system, instead of working to realize the intent of Congress to broaden the institutional and geographic base of scientific research in this country, has worked to create a tightly knit "old boy network."

In the National Science Foundation Act of 1950, Congress explicitly directs, and I quote, "It shall be an objective of the Foundation to strengthen research and education in the sciences, including independent research by individuals, throughout the United States, and to avoid undue concentration of such research and education." Yet, in practice, the NSF and the system of peer review have produced precisely the undue concentration of scientific research and education that the Congress has been concerned to avoid.

In the article on "Peer Review and the Public Interest" to which I referred earlier, the authors, Dr. Atkinson and Dr. Blanpied, frequently refer to the Congress' recent actions in providing funds for new science facilities as "pork barreling." The evidence clearly shows that the real pork barrel in scientific research is the system that benefits the very research universities that have been loudest in claiming the purity of peer review. And that system is operated by the same agencies that have joined in the hue and cry.

In a very important footnote, Drs. Atkinson and Blanpied assert, and I quote, "The opinions of the authors are their own and do not necessarily reflect the policies of their institutions." Their attempt to use the immense prestige of the NSF while lamely protesting that they speak only for themselves is, in my opinion, as transparent as Salome's seventh and last veil.

It is worth describing briefly how the present situation came about. In the beginning, there were few institutions that were able to provide outstanding facilities for scientific research on their own, either through endowment funds or through liberal access to State tax dollars. Obviously, such facilities attract outstanding research scientists and outstanding students who together produce outstanding scientific work.

This is the reason that academically strong institutions such as Boston University, lacking the endowment funds or the access to State funds to build equivalent facilities, have sought assistance from the Congress. These institutions have demonstrated their ability to contribute to the national effort in scientific research, and they properly resent being shut out by those who got to the trough first. The Nation would also suffer from their exclusion, had not Congress wisely decided to support the development of alternative sites for scientific work.

But those institutions that were first to establish their excellence have shortsightedly decided that they shall restrict membership in the scientific establishment to themselves. Possessing a group of distinguished investigators, it is natural that support for individual projects is distributed through the peer review process to the outstanding scientists already employed by the outstanding established institutions.

In addition, many scientists who are invited to serve a term as project officers within the granting agencies come from the very same institutions. The consequence is that rich and well established universities and their distinguished faculties, both on campus and in the agencies, become the judges in their own cases. And when one becomes a judge in his own case, that violates a fundamental principle of democratic society, a principle that we got from John Locke that no man and no institution should be a judge in its own case.

In these circumstances, it is hardly surprising that the rich get richer, or that the disadvantaged seek relief from Congress. As long as we can deny adequate facilities to the have-not universities, we can deny them the ability to attract outstanding scientists. And, in the absence of outstanding scientists, they don't have a chance in peer review.

So, those who are already very successful in the peer review process can sustain their monopoly quite simply, by simply cutting off facilities development in the institutions that are not yet developed in that area. Once an institution that is a have-not institution develops its facilities, however, then it can attract the very finest scientists right along with those other well established institutions, and those outstanding scientists can successfully compete for the peer review dollars.

The Congress was created in part to prevent exclusionary practices. For, despite the intentions of Congress, Federal policies toward scientific research have operated to create in effect two castes of research universities, and it is not merely appropriate but in conformity with national goals established by Congress for the have-not institutions to seek Federal support to redress the imbalance that currently exists.

Nor should anyone be surprised by the strong defensive reaction of the have institutions to the initiatives of the have nots. As we have seen, the haves constitute an informal cartel, and they behave much like classical economic cartels such as OPEC. Like OPEC, it is in the interest of the members of this academic cartel to preserve the status quo, because it confers so many benefits upon them.

In consequence, they have reacted violently to the appearance of a mechanism—namely, direct congressional funding—which, if it were to become widespread, would threaten to break the cartel by placing other institutions in a position to compete on an equal or nearly equal footing for the limited Federal research funds dispensed by peer review.

Those institutions seeking direct congressional assistance are by no means trying to avoid peer review in the only area in which it has traditionally operated—namely, basic research—but only to achieve the critical mass of facilities and personnel necessary to compete in that context of peer review.

Moreover, the haves have adopted a strategy that has the same effect as direct congressional funding, but because it is less visible to the public, it has not generally been recognized for what it is. A number of them have actively lobbied for bills that would fund specific new programs in the budgets of the Federal agencies, and they have been very careful to inform the agencies, and the staffs of those agencies, of their efforts on behalf of those programs.

When the bills pass and the new programs are established, the universities that have been successful in establishing those funds then apply for grants from these new funds; and by a strange and marvelous coincidence, they are the ones who receive the grants.

The difference between this practice and the practice of directly approaching Congress for funds for specific projects is difficult to determine, except that the direct approach to the Congress is more public. It seems obvious that, for the academic cartel, what is sauce for the goose is definitely not sauce for the gander.

An example of how this strategy works can be found in the recent NSF Program to distribute supercomputers to a number of universities. The funds for this program were added to the NSF budget in large measure through the lobbying efforts of major universities. But their lobbying did not end there, even though one might imagine that they would be especially scrupulous about avoiding the use of political pressure to influence a peer reviewed project.

Not so. Even after the program had been approved, the lure of the extraordinarily expensive supercomputers provoked a frenzy of lobbying to obtain them, lobbying by very distinguished scientists. The successful efforts of two distinguished universities, with distinguished scientific faculties, were chronicled in an instructive story in the New York Times on March 16, 1985.

It is important to reemphasize that this was not a project involving the construction of science facilities, the sort of project that has never been the subject of the peer review process used to award grants to individual investigators; this was a project to establish supercomputer centers at several locations around the country by providing the supercomputers themselves as well as a substantial amount of support—and it was, at least in theory, subject to peer review.

By this point in my testimony, I expect no one will be startled to learn that all four supercomputers distributed by the NSF went to institutions that were already among the top 20 in the NSF funding.

The NSF Supercomputer Program also raises another important issue bearing on the question of science in the political process. These grants, which average $50 million apiece—or more than double the assistance Boston University received from the Federal Government—will inevitably have a major economic impact on the communities and regions to which they are given.

Nor has this fact been ignored by the universities that lobbied successfully for the supercomputers. In an article in the New York Times of April 21, 1985, officials of one of the universities whose lobbying efforts I referred to earlier are quoted at length on the economic benefits of the supercomputer. Indeed, they said, "they hoped the supercomputer center * * * would serve as a centerpiece for coordinated, statewide economic development efforts." As one expert said, "It's clear that colossal computers won't be available in many places. If you offer them to private industry, it is bound to foster economic expansion."

These are remarkable statements because they constitute an explicit recognition of the reality that Federal investments in science can have consequences that go far beyond the merely scientific. They can have economic and business implications and research implications way beyond science.

I should like to point out, however, the glaring inconsistency that exists between statements such as these and the criticisms that the "have" institutions feel free to make of the much more modest Federal investments in facilities for "have-not" institutions. Nowhere in those criticisms will one find any acknowledgment of the important economic and social consequences of those investments.

In my testimony before the Committee on Science and Technology last May, I demonstrated the contribution that Boston University's new Science and Engineering Center will make both to the revitalization of a blighted urban area and to the new high technology industries of Massachusetts and New England. These are aspects which, if they are to be subjected to peer review, would not be subjected to the review of scientists but to people outside the area of science—to business people, to managers, to experts in economics.

I also described the woeful inadequacy of the facilities and equipment available for scientific and technological research in America's colleges and universities in general. I emphasized the point that has also frequently been made by members of this task force: that our country's well-being and perhaps even its survival depends upon the development of a national science policy that will permit us to refurbish and renew our obsolete and obsolescent scientific infrastructure.

Yet the critics of direct congressional funding of scientific facilities refuse to deal with these arguments, because if they did they would be forced to admit that the issues of local, regional, and national economic and technological development lie far beyond their competence, and lie directly within the competence of the Members of Congress.

The location of federally supported research facilities has an enormous beneficial impact on the economy of the host communities. Not only do such projects have the short-term effect of creating construction jobs, but they have the crucial long-term effect of attracting industries with related interests.

When a research project involves significant initial costs, when critical national research interests are at stake, and when a host community will be affected economically, socially, or environmentally, then Congress is the only appropriate body to decide how Federal tax dollars should best be spent.

In fact, the volume of direct Federal funding for scientific facilities—about $100 million over the past 3 years—represents an extremely modest and limited exercise of Congress' undoubted authority to determine the allocation of Federal research dollars. The amount does not begin to meet the national need for university research facilities.

In this context, it is clear that universities that approach the Congress directly for funds with which to help meet our national need for adequate research facilities are behaving precisely as the framers of the Constitution intended when they guaranteed the right to petition for redress of grievances. Such approaches are among the most effective ways to bring to the attention of Congress the crisis in science facilities that looms on the horizon.

Attempts by private organizations and Federal science agencies to discourage institutions from approaching the Congress directly, and to discipline institutions that have done so, are not merely attempts to abridge a right ensured by the Constitution; they are also selfish, shortsighted, and contrary to the national interest. In addition, I think they are self-defeating.

Drs. Atkinson and Blanpied have gone so far as to suggest in their article that institutions and individuals approaching the Congress directly be, and I quote, "effectively censured . . . by being threatened with denial of subsequent support by official peer panels." It seems not to have occurred to them that using peer review for disciplinary purposes would itself destroy the integrity of the peer review process by undermining its credibility and objectivity.

Mr. Chairman, I wish to make clear that in defending the concept of direct approaches to Congress for facilities funding, I do not wish in any way to denigrate the peer review system when it is properly used to ensure that grant proposals from individual investigators are evaluated by the most competent scientists available.

Boston University is hardly an unsuccessful suitor in the peer review process. Each year our faculty receives approximately $40 million in competitive, peer reviewed grants and contracts. Nevertheless, in light of the clear and unveiled threat that I have just quoted, I must report that I had some misgivings about accepting your invitation to testify before this task force.

I believe it is in the national interest for me to bring to your attention the facts that I have presented. But it would be disastrous for Boston University—and a waste of Federal and other investments that have been made toward its development as a center for scientific excellence—if our research support were to suffer as a consequence of my testimony, and if the peer reviews before which our faculty must go will follow the advice of those authors and threaten us with denial of subsequent support.

Unfortunately, serious damage can not only be done surreptitiously, but can also be justified in language that cloaks the real intent with apparent high-mindedness. I trust that the task force is already aware of the context of intimidation in which I have presented my testimony and is prepared to take the steps necessary to ensure that no untoward consequences follow.

The intellectual myopia and personal and professional vindictiveness that have obscured the issue of science in the political process provide additional evidence, if more were needed, that we should be glad our system of government was built on twin principles that no man shall be a judge in his own case and that the people, acting through their elected representatives, shall be the final arbiters of the common good.

If we encourage misguided attempts by some members of the scientific community to abrogate these principles, we shall find not only our science policy but our entire society in disarray.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Silber follows:]

## PREPARED STATEMENT OF DR. JOHN R. SILBER, PRESIDENT, BOSTON UNIVERSITY

Chairman Fuqua and members of the Science Policy Task Force:

I am very pleased to have this opportunity to discuss the place of "Science in the Political Process." Not only is this an issue with important implications for federal policy regarding science and higher education, it is also an important issue from the point of view of political philosophy.

The role of science and scientists in politics is a specific instance of a wider question that has been debated by philosophers since at least the 5th century B.C. Broadly stated, that question is whether we should be ruled by experts. Plato, in the Republic, sketched out an elaborate educational scheme designed to maximize the potential of each member of society. But only the most successful recipients of this education would acquire the full knowledge and virtue which, in his view, were required of rulers. The remainder, those without the natural ability to enter the class of rulers, would fall naturally into other categories and in these roles would make important and even essential contributions to their society. But they would be properly subordinated to those qualified by talent and education to know what was best for everyone else.

Many philosophers since Plato have been drawn to the vision of a fully just society as one in which each individual is given the opportunity--an equal opportunity uninfluenced by wealth or family circumstance--to reveal his inclinations and to develop his capacities through a system of universal education. Indeed, this aspect of Plato's thought made a major impact on Jefferson and our other Founding Fathers, who held that each individual has an inalienable right to life, liberty and the pursuit of happiness. From Plato came the firm belief of Jefferson and Adams that these rights can be achieved only if education is available to each individual. But most philosophers have been reticent in proposing schemes to rebuild society in a way that forces everyone to participate in a comprehensive system for education and social classification, for they have rightly recognized that this would entail the creation of a totalitarian state and thereby destroy that core of individual freedom on which equality of opportunity depends. The concern to preserve human freedom was of course central to the work of the framers of our Constitution.

Perhaps the most devastating critique of Plato's apotheosis of the expert can be found in the work of his younger contemporary, Aristotle. In the Politics, Aristotle observes that

> if the people are not utterly degraded, although individually they may be worse judges than those who have special knowledge--as a body they are as good or better. Moreover there are some arts whose products are not judged solely, or best, by the artists themselves, namely those arts whose products are recognized even by those who do not possess the art; for example, the knowledge of the house is not limited to the builder only; the user . . . of the house will even be a better judge than the builder, just as the pilot will judge better of a rudder than the carpenter, and the guest will judge better of a feast than the cook. (1282ª [Book III, Chapter 11]; tr. by Benjamin Jowett)

Aristotle here succinctly expresses a fundamental principle of American democracy: that in matters affecting society as a whole, those whose lives are affected--the people--are likely to be better judges than any class of experts. It is for this reason that the Constitution reserves to the Congress, to the elected representatives of the people, not only the power to declare war but also the power levy taxes and to judge for what purposes that money shall be used. Just as in providing for the common defense the people through their elected representatives shall decide what they shall be defended against, so also in promoting the general welfare the people shall determine through their elected representatives what is truly in their best interests.

This does not mean that the opinions of experts have no value or that they should be disregarded entirely. It does mean, however, that the Congress cannot delegate to experts its ultimate responsibility for promoting the general welfare. Legislators who must decide on issues that can be illuminated by experts have an obligation to seek out and listen to those who are best qualified to advise them. But under our system of government, the only competent experts on what constitutes the common good are the people themselves, acting through their elected representatives.

This, in my view, is exactly as it should be. Being the best cardiac surgeon in the world makes one an expert on the diseases of the heart and on some of the ways to cure such diseases. But no surgeon, however skillful, is qualified simply because of his professional skill to pronounce on whether society as a whole would be better off spending millions to perfect the artificial heart rather than spending that same money to reduce the national deficit. In fact, common sense suggests that our hypothetical surgeon, precisely because of his concentration on cardiology, may be less able to judge objectively among competing goods when one of those goods is the very thing to which he has devoted his life. This is the psychological truth behind Aristotle's observation that as judges of their own welfare the people are at least as good and often better than those who have special knowledge.

In this context, much of the confusion that has obscured the subject of "science in the political process" disappears. Do federal investments in science involve merely technical scientific issues? Obviously not. Every thinking person knows that the massive federal support for science and technology that began during World War II and that has continued up to the present reflects America's rise to world leadership and our national need to retain military and industrial primacy in the face of the military threat posed by the Soviet Union and the economic and industrial threat posed by Japan and other nations.

Over the past forty years, the intellectual geography of American science has been transformed in response to national priorities. There were only a handful of elementary-particle physicists working in the United States before the Second World War. By the late 1970s, largely as a result of this field's importance for our national security, there were nearly 2,000. (High Energy Physics Advisory Panel, Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics Program, Washington, U.S. Department of Energy, 1980; cited in Andrew Pickering, Constructing Quarks, Chicago, The University of Chicago Press, 1984.) A similar explosive growth occurred in space science in consequence of our commitment to the space program, a decision made largely on political and strategic grounds. Ninety-six percent of the federal dollars spent to support the 19 Federally Funded Research and Development Centers go to just 10 FFRDCs, concentrated in the areas of weapons research, space research, high-energy particle physics and plasma physics. (National Science Foundation, Federal Support to Universities, Colleges, and Selected Nonprofit Institutions, Washington, March, 1984, p. 25.)

This point is made even clearer when we consider how federal funds are distributed between applied and basic research. Of the $50.9 billion in federally-sponsored research in FY 1985, approximately $43.1 billion is dedicated to specific, mission-oriented applied research, while $7.8 billion is dedicated to basic research. While one might quarrel with these proportions, it would be absurd to argue that they somehow reflect a "natural" distribution based only on the expert judgment of scientists. Rather, they reflect the judgment of the Congress based in large measure on considerations of national security and economic and industrial growth.

We should therefore not allow ourselves to be misled by claims implying that Congress is somehow shattering a time-honored precedent if it invests public monies in scientific research only after considering the full range of issues that affect the public interest. The very structure of American science is itself the result of a long and generally consistent series of such investment decisions made by the Congress since the end of World War II.

It must be emphasized again that under our system of government, this is as it should be.

In recent months, however, certain sectors of the scientific community, including a quintessential special-interest group, the trade association for the major research universities, have engaged in much public hand-wringing. The pretext for these cries of alarm is not a revolutionary change in Congressional policy towards scientific research. Instead, it is claimed to arise from just 15 Congressional actions, amounting to a total appropriation of approximately $100 million, to help 15 universities build new or improved facilities. The National Science Board claims that all of these projects involve facilities for scientific research, but that claim is not accurate. Several of the projects they cite involved instructional or training facilities having nothing to do with scientific research, and all the facilities in question are mixed-use facilities not strictly research facilities.

Boston University, as most members of the Task Force already know from my testimony before the Committee on Science and Technology on May 8, 1984, was one of the universities that received direct Congressional funding. We were granted $19 million in federal funds to assist in the development of a new Science and Engineering Center. The total cost of this project is approximately $90 million, three quarters of which will be financed by industry, by philanthropy, and by Boston University itself. I refer members of the Task Force to my earlier testimony for the details of this project, as well as for additional views bearing on the subject of this morning's hearing.

It might be supposed that any objections to such projects from within the scientific and academic community would stem from doubts (perhaps even supported by some evidence) that the research performed in these new facilities would measure up to the highest standards of scientific endeavor. Not at all. Not one scintilla of evidence has ever been adduced to support such a view, and the authors of recent article on "Peer Review and the National Interest," a former Director and a current senior employee of the National Science Foundation, explicitly say that "the point at issue is not whether meritorious research will be carried out in facilities" obtained by direct Congressional appropriation. (Richard C. Atkinson and William A. Blanpied, Issues in Science and Technology, Summer 1985, pp. 101-114.)

Instead, virtually all--and certainly the most strident--of the objections that have been raised against federal appropriations for new science facilities rest upon a far odder argument: that by providing this minimal aid to renew the nation's obsolete and obsolescent scientific infrastructure the Congress is somehow undermining the system of "peer review."

This argument is false on its face. In the first place, it completely ignores the fact that funds to build the facilities in which scientists work have for the most part never been subject to the traditional peer review that is applied to proposals from individual investigators for basic research grants. In addition, the new facilities built with federal funds not only will not diminish the effectiveness of the peer review system, but will actually increase its scope.

We should first be clear about the limited extent of peer review in federally funded research. The peer review system does not, for example, apply to the 19 Federally Funded Research and Development Centers, which are typically owned by the federal government and operated by host universities under contract arrangements with the sponsoring federal agency. Funds for these federal laboratories, which have amounted to more than $2.5 billion in each of the past three years, appear as a series of line items in the federal budget and are not subject to peer review.

Nor does the peer review system apply to most of the research funds distributed by the U.S. Department of Agriculture. Most USDA research funds are distributed to state land-grant colleges and universities by means of formulas that do not consider the academic quality of the research but rely rather on the relative size of the rural and farm populations in the states where these institutions are located. In FY 1985, the Department of Agriculture will fund nearly $940 million in research, approximately half of which will be conducted at universities. Only $48 million, or about 5%, is subject to peer review.

In fact, the peer review system, which uses panels of outside evaluators to rank competing proposals for limited research funds on the basis of merit, operates mainly in the National Science Foundation and the National Institutes of Health. A somewhat similar system is also in place for nuclear and high-energy physics research funded by the Department of Energy. This, of course, is just what one might expect. These agencies and programs are primarily designed to provide grants to individual investigators to support basic research. Their purpose and organization are thus better suited to the use of external advisory panels to comment on applications from university-based researchers than mission-oriented agencies like NASA, the Department of Defense, and even the Department of Agriculture.

Because federal support of science and technology is spread over so many agencies and because the mechanisms for providing that support are so various, it is difficult to determine precisely how much federally-sponsored scientific research is actually subject to peer review. A conservative estimate, however, would be that no more than $4 billion out of $50.9 billion, or less than 8¢ of every federal dollar spent on science and technology, falls under the peer review system. And almost all of the research support that is awarded through peer review comes to university faculty members and is carried on in laboratories and other facilities located on university campuses.

When Congress spends funds to establish additional centers or to upgrade the quality of existing centers, it increases the number and quality of places in which work is done that appropriately falls under the peer review system. By increasing the university-based facilities in which high quality scientific research can be performed, Congress thereby increases the scope of the peer review system. This should be an occasion for rejoicing within the scientific community. Yet, as we have seen, a few scientists, as well as some members of the federal scientific bureaucracy and some major research universities, are not rejoicing. Why?

To understand this apparently mysterious phenomenon, it is necessary to pierce the veil of self-serving rhetoric that surrounds the peer review system. No one would deny that the principle of peer review is commendable, when it is applied in the appropriate way to appropriate programs. In practice, however, the system of peer review now in operation in this country is seriously flawed. Approximately 20 institutions, clustered in only three geographic regions, receive nearly half of all federal research support. In FY 1983, for example, 20 institutions received approximately 40% of the total awarded to all 592 research universities receiving federal research and development support. In FY 1983, the top 10 institutions receiving support from the NSF received 30% of all NSF funds, and the top 20 institutions received 46% of all NSF funds. The top twenty recipients of NIH support have been the same institutions for the past ten years.

The peer review system, instead of working to realize the intent of Congress to broaden the institutional and geographic base of scientific research in this country, has worked to create a tightly knit old-boy network. In the National Science Foundation Act of 1950 (P.L. 81-507; 42 USC 1861), Congress explicitly directs that "it shall be an objective of the Foundation to strengthen research and education in the sciences, including independent research by individuals, throughout the United States, and to avoid undue concentration of such research and education." Yet in practice the NSF and the system of peer review have produced precisely the undue concentration of scientific research and education that the Congress has been concerned to avoid.

In the article on "Peer Review and the Public Interest" to which I referred earlier, the authors, Drs. Atkinson and Blanpied, frequently refer to the Congress's recent actions in providing funds for new science facilities as "pork barreling." The evidence clearly shows that the real pork barrel in scientific research is the system that benefits the very research universities that have been loudest in claiming the purity of peer review. And that system is operated by the same agencies that have joined in the hue and cry. (In a footnote, Drs. Atkinson and Blanpied assert that "[t]he opinions of the authors are their own and do not necessarily reflect the policies of their institutions." This attempt to use the immense prestige of the NSF while lamely protesting that they speak only for themselves is as transparent as Salome's seventh veil.)

It is worth describing briefly how the present situation came about. In the beginning, there were a few institutions that were able to provide outstanding facilities for scientific research on their own, either through endowment funds or through liberal access to state tax dollars. Obviously, such facilities attract outstanding research scientists and outstanding students who together produce outstanding scientific work. This is the reason that academically strong institutions lacking the endowment funds or state largesse to build equivalent facilities have sought assistance from the Congress. These institutions have demonstrated their ability to contribute to the national effort in scientific research, and they properly resent being shut out by those who got to the trough first. The nation would also suffer from their exclusion, had not Congress wisely decided to support their development.

But those institutions that were first to establish their excellence have shortsightedly decided that they shall restrict membership in the scientific establishment to themselves.

Possessing a group of distinguished investigators, it is natural that support for individual projects is distributed through the peer review process to the outstanding scientists already employed by established institutions. In addition, many scientists who are invited to serve a term as project officers within the granting agencies come from the same universities. The consequence is that rich and well established universities and their distinguished faculty--both on campus and in the agencies--become judges in their own cases.

In these circumstances, it is hardly surprising that the rich get richer--or that the disadvantaged seek relief from the Congress. The Congress was created in part to prevent such exclusionary practices. For despite the intentions of Congress, federal policies towards scientific research have operated to create in effect two castes of research universities, and it is not merely appropriate but in conformity with national goals established by Congress for the "have-not" institutions to seek federal support to redress the imbalance that currently exists.

Nor should anyone be surprised by the strong defensive reaction of the "have" institutions to the initiatives of the "have nots." As we have seen, the "haves" constitute an informal cartel, and they behave much like classical economic cartels, such as OPEC. Like OPEC, it is in the interest of the members of this academic cartel to preserve the status quo, because it confers so many benefits upon them. In consequence, they have reacted violently to the appearance of a mechanism--direct Congressional funding--which if it were to become widespread would threaten to break the cartel by placing other institutions in a position to compete on an equal or nearly equal footing for the limited federal research funds dispensed by peer review. Those institutions seeking direct Congressional assistance are by no means trying to avoid peer review in the only area in which it has traditionally operated--basic research--but only to achieve the critical mass of facilities and personnel to compete in such a context.

Moreover, the "haves" have adopted a strategy that has the same effect as direct Congressional funding, but because it is less visible to the public has not generally been recognized for what it is. A number of them have actively lobbied for bills that would fund specific new programs in the budgets of federal agencies, and have informed the agencies of their efforts. When the bills pass and the new programs are established, the universities have then applied for grants from the new funds. By a strange coincidence, the grants have been forthcoming. The difference between this practice and the practice of directly approaching Congress for funds for specific projects is difficult to determine. It seems obvious that, for the academic cartel, what is sauce for the goose is definitely not sauce for the gander.

An example of how this strategy works can be found in the recent NSF program to distribute supercomputers to a number of universities. The funds for this program were added to the NSF budget in large measure through the lobbying efforts of major universities. But their lobbying did not end there, even though one might imagine that they would be especially scrupulous about avoiding the use of political pressure to influence a peer reviewed project. Not so. Even after the program had been approved, the lure of the extraordinarily expensive supercomputers provoked a frenzy of lobbying to obtain them. The successful efforts of two distinguished universities were chronicled in an instructive story in the New York Times on March 16, 1985.

It is important to re-emphasize that this was not a project involving the construction of science facilities, the sort of project that has never been subject to the peer review process used to award grants to individual investigators; this was a project to establish supercomputer centers at several locations around the country by providing the supercomputers themselves as well as a substantial amount of support--and it was, at least in theory, subject to peer review. By this point in my testimony, I expect no one will be startled to learn that all four supercomputers distributed by the NSF went to institutions that were already among the top 20 in NSF funding.

The NSF supercomputer program also raises another important issue bearing on the question of "science in the political process." These grants, which average $50 million each, or more than double the assistance Boston University received from the federal government, will inevitably have a major economic impact on the communities and regions to which they are given. Nor has this fact been ignored by the universities that lobbied successfully for supercomputers. In an article in the New York Times of April 21, 1985, officials of one of the universities whose lobbying efforts I referred to earlier are quoted at length on the economic benefits of the supercomputer. Indeed, "they hoped the supercomputer center . . . would serve as a centerpiece for coordinated, statewide economic development efforts." As one expert said, "It's clear that colossal computers won't be available in many places. If you offer them to private industry it is bound to foster economic expansion."

These are remarkable statements, because they constitute an explicit recognition of the reality that federal investments in science can have consequences that go far beyond the merely scientific. I should like to point out, however, the glaring inconsistency that exists between statements such as these and the criticisms that the "have" institutions feel free to make of the much more modest federal investments in facilities for "have-not" institutions.

Nowhere in those criticisms will one find any acknowledgement of the important economic consequences of those investments. In my testimony before the Committee on Science and Technology last May I demonstrated the contribution that Boston University's new Science and Engineering Center will make both to the revitalization of a blighted urban area and to the new high technology industries of Massachusetts and New England. I also described the woeful inadequacy of the facilities and equipment available for scientific and technological research in America's colleges and universities. I emphasized a point that has also frequently been made by members of this Task Force: that our country's well-being and even perhaps its survival depend upon the development of a national science policy that will permit us to refurbish and renew our obsolete scientific infrastructure.

Yet the critics of direct Congressional funding of scientific facilities refuse to deal with these arguments, because if they did they would be forced to admit that issues of local, regional, and national economic and technological development lie far beyond their competence--and lie directly within the competence of Congress. The location of federally-supported research facilities has an enormous beneficial impact on the economy of the host communities. Not only do such projects have the short-term effect of creating construction jobs, they also have the crucial long-term effect of attracting industries with related interests.

When a research project involves significant initial costs, when critical national research interests are at stake, and when a host community will be affected economically, socially, or environmentally, then Congress is the only appropriate body to decide how federal tax dollars should best be spent. In fact, the volume of direct federal funding for science facilities--about $100 million over the past three years--represents an extremely modest and limited exercise of Congress's undoubted authority to determine the allocation of federal research dollars. The amount does not begin to meet the national need for university research facilities.

In this context, it is clear that universities that approach Congress directly for funds with which to help meet our national need for adequate research facilities are behaving precisely as the framers of the Constitution intended when they guaranteed the right to petition for redress of grievances. Such approaches are among the most effective ways to bring to the attention of Congress the crisis in science facilities that looms on the horizon.

Attempts by private organizations and federal science agencies to discourage institutions from approaching Congress directly, and to discipline institutions that have done so, are not merely attempts to abridge a right ensured by the Constitution, they are also selfish, shortsighted and contrary to the national interest. In addition, they are self-defeating. Drs. Atkinson and Blanpied have gone so far as to suggest in their article that institutions and individuals approaching Congress directly be "effectively censured . . . by being threatened with denial of subsequent support by official peer panels." It seems not to have occurred to them that using peer review for disciplinary purposes would itself destroy the peer review process by undermining its integrity, credibility and objectivity.

Mr. Chairman, I wish to make it clear that, in defending the concept of direct approaches to Congress for facilities funding, I do not wish in any way to denigrate the peer review system when it is properly used to ensure that grant proposals from individual investigators are evaluated by the most competent scientists available. Boston University is hardly an unsuccessful suitor in the peer review process: each year, our faculty receives approximately $40 million in competitive, peer-reviewed grants and contracts.

Nevertheless, in light of the unveiled threat I have just quoted, I must report that I had some misgivings about accepting your invitation to testify before the Task Force. I believe it is in the national interest for me to bring to your attention the facts that I have presented. But it would be disastrous for Boston University--and a waste of the federal and other investments that have been made towards its development as a center of scientific excellence--if our research support were to suffer as a consequence of my testimony. Unfortunately, serious damage can not only be done surreptitiously but also justified in language that cloaks the real intent with apparent high-mindedness. I trust that the Task Force is already aware of the context of intimidation in which I have presented my testimony, and is prepared to take the steps necessary to ensure that no untoward consequences follow.

The intellectual myopia and personal and professional vindictiveness that have obscured the issue of "science in the political process" provide additional evidence, if more were needed, that we should be glad our system of government was built upon the twin principles that no man shall be judge in his own case and that the people, acting through their elected representatives, shall be the final arbiters of the common good. If we encourage misguided attempts by some members of the scientific community to abrogate these principles, we shall find not only our science policy but our entire society in disarray.

A14 Wednesday March 6, 1985

# Schools Accused of Evading Reviews for U.S. Financing

By PHILIP M. BOFFEY

Special to The New York Times

WASHINGTON, March 5 — A top Federal science board has charged that 15 universities in the last two years created "a dangerous precedent" by circumventing usual merit reviews and obtaining more than $100 million directly from Congress to build laboratory facilities.

This, the National Science Board said, is "the wrong solution to a real and urgent problem" of deteriorating research facilities.

In several cases, the board said the money obtained directly from Congress was diverted "from other scientific activities selected on the basis of their merit."

In the normal procedure for getting Federal help to build scientific facilities, universities submit their requests to a Federal agency and the plan's merit is then rated by scientists familiar with the area. Based on this, the agency decides what projects to support and requests appropriations.

### Board Makes Agency Policy

The board, composed of leading scientists and engineers from industry and the universities, is the policy-making body for the National Science Foundation, the Government's chief agency for supporting basic scientific research. It frequently comments on science problems that affect the entire Government and scientific community.

The board's statements were made after it endorsed a report on the problem from a panel headed by its vice chairman, Charles E. Hess, dean of agriculture and environmental sciences at the University of California at Davis.

The committee noted that some universities had hired "professional lobbyists" and many "exerted pressure on their representatives."

"If this becomes common," the committee warned, "it could seriously undermine the U.S. system of merit competition for research funding that has been so successful."

The committee acknowledged that political lobbying played a standard role in decisions by Congress on highways, mass transit and dams. But it asserted that most Federal financing for science, particularly basic research, "has enjoyed considerable freedom from special-interest politics" and decisions had been made after merit reviews.

The board called for a conference on "politically feasible solutions."

### It Lists the Schools Involved

The board made public a list of 15 universities that had persuaded Congress to appropriate or authorize money for facilities in the last two years without the endorsement of review groups. Some got the money under amendments attached to bills at the last minute by cooperative Members of Congress, thus bypassing even committee debate.

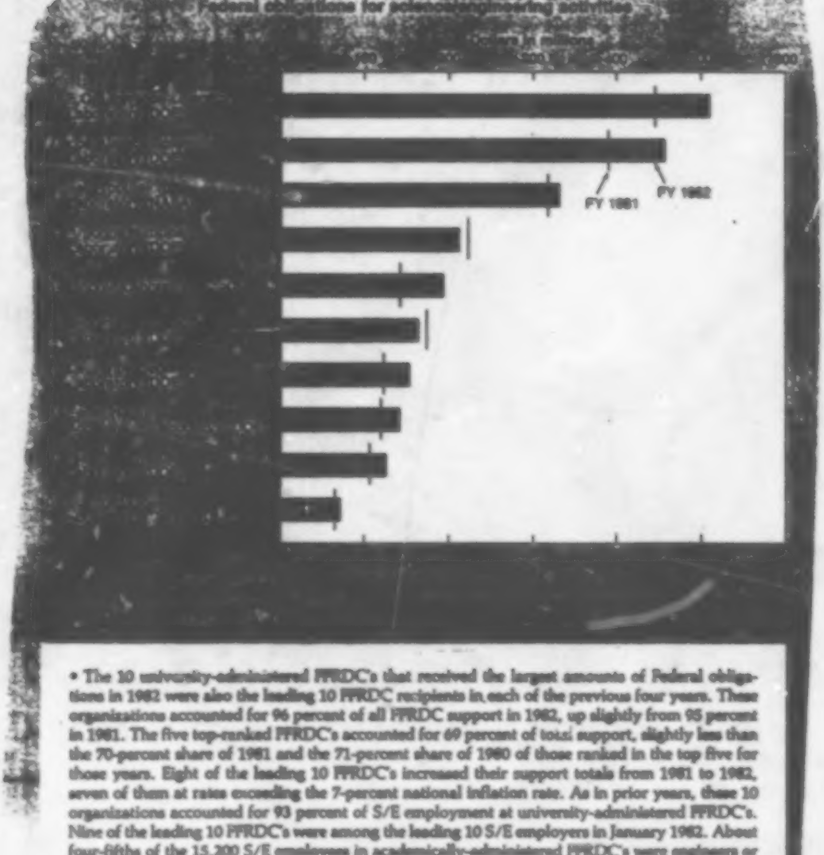
Here are 12 of the names on the list:

Catholic University, $13.9 million for a vitreous state laboratory;
Columbia, $8 million for a chemical research laboratory;
University of New Hampshire, $15 million for a space and marine science building;
Florida State, $7 million for a supercomputer center;
Northwestern, $16 million for a basic industry research institute;
Boston University, $19 million for an engineering building;
West Virginia University, $4.5 million for a cancer research center;
University of North Carolina, $800,000 to expand undersea research;
University of Connecticut, a pediatric research and training center to get part of a $1.5 million appropriation;
University of Hawaii, a rehabilitation research and training center to get part of a $1.5 million appropriation;
University of Oregon, $2.3 million for a science facility;
University of Kansas, $9 million for a human development center.

In addition, Indiana University was authorized $6 million for a center for educational excellence and the University of Utah $4 million for a research center on the health effects of nuclear energy, although it is not yet clear whether these appropriations will be approved. Congressional conferees urged that the University of New Mexico get "high priority" for a new $18.2 million engineering laboratory if Congress appropriates money for a construction grants program.

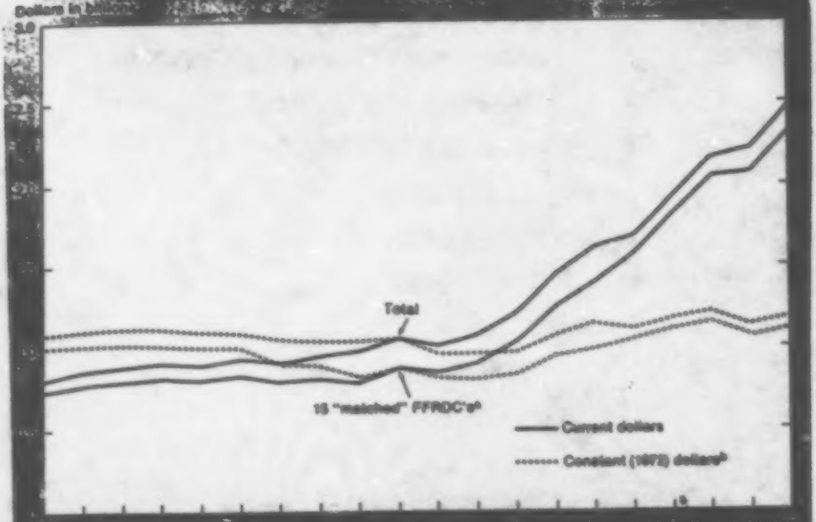
Seven other institutions received financing for facilities that the board did not consider crucial for basic research.

Chart 21. University-administered FFRDC's receiving the largest amounts of Federal obligations for science/engineering activities



• The 10 university-administered FFRDC's that received the largest amounts of Federal obligations in 1982 were also the leading 10 FFRDC recipients in each of the previous four years. These organizations accounted for 96 percent of all FFRDC support in 1982, up slightly from 95 percent in 1981. The five top-ranked FFRDC's accounted for 69 percent of total support, slightly less than the 70-percent share of 1981 and the 71-percent share of 1980 of those ranked in the top five for those years. Eight of the leading 10 FFRDC's increased their support totals from 1981 to 1982, seven of them at rates exceeding the 7-percent national inflation rate. As in prior years, these 10 organizations accounted for 93 percent of S/E employment at university-administered FFRDC's. Nine of the leading 10 FFRDC's were among the leading 10 S/E employers in January 1982. About four-fifths of the 15,200 S/E employees in academically-administered FFRDC's were engineers or physical scientists.[1]

Chart 10. Federal obligations to university-administered FFRDC's for science/engineering activities [illegible]

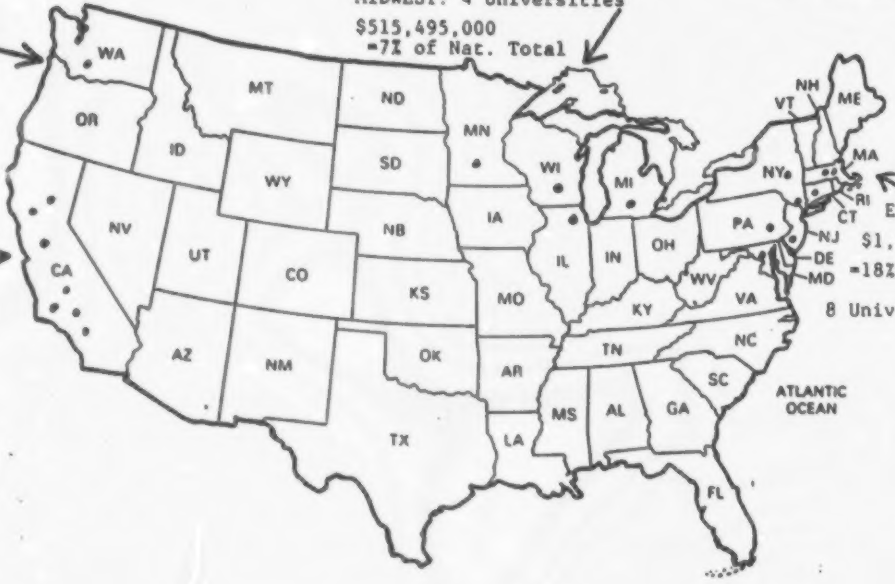


[redacted] received $2.5 billion in 1982 [redacted] (4 percent in real dollars). About five-sixths of all FFRDC support [redacted] projects. The 15 FFRDC's that were in existence throughout the entire 1963-82 period accounted for 94 percent of all support for all university-administered FFRDC's and for virtually all of the 1981-82 funding growth to these organizations. These 15 centers accounted for at least 80 percent of the total funding for all academic FFRDC's in each year during the 1963-82 survey period. From 1982 to 1984, R&D and R&D plant funds for academic FFRDC's are expected to average a 5-percent annual growth in current dollars and reach a level of $2.7 billion.[3]

BEST COPY AVAILABLE

## FEDERAL FUNDING FY 83
## TOP 20 RECIPIENTS

National Total $7,776,855
Total Top 20 4,306,298
=55% of National Total


WA
OR
CA
ID
NV
MT
WY
UT
AZ
CO
NM
ND
SD
NB
KS
OK
TX
MN
IA
MO
AR
LA
WI
IL
MI
IN
OH
KY
TN
MS
AL
GA
FL
SC
NC
VA
WV
PA
NY
VT
NH
ME
MA
RI
CT
NJ
DE
MD
PACIFIC OCEAN
ATLANTIC OCEAN

WEST
$114,294
=1% of Nat. Total
1 University

MIDWEST: 4 Universities
$515,495,000
=7% of Nat. Total

CALIFORNIA:
$2,289,642
=29% of Nat. Total
7 Universities

EAST:
$1,386,867,000
=18% of Nat. Total
8 Universities

## NSF FUNDING PATTERNS
## TOP 20 RECIPIENTS

| RANK COLLEGE/ UNIVERSITY | NSF FUNDS FY83 | 1985 NSF AWARDS SUPER COMP. | ENGINEER CTRS | AAU MBR |
|---|---|---|---|---|
| 1. U.C.-San Diego | 37,863,000 | Yes | | Yes |
| 2. Cornell | 34,129,000 | Yes | | Yes |
| 3. MIT | 33,860,000 | | Yes | Yes |
| 4. U.IL-Urbana | 21,526,000 | Yes | | Yes |
| 5. Stanford | 21,522,000 | | | Yes |
| 6. Columbia | 21,253,000 | | Yes | Yes |
| 7. U.C.-Berkeley | 19,141,000 | | | Yes |
| 8. U. Washington | 17,872,000 | | | Yes |
| 9. U.WI-Madison | 17,682,000 | | | Yes |
| 10. CA Tech | 15,489,000 | | | Yes |
| TOTAL TOP 10 30% of NSF Funds | 240,337,000 | | | |
| 11. U. Michigan | 15,368,000 | | | Yes |
| 12. Harvard | 14,330,000 | | Yes | Yes |
| 13. U.C.L.A. | 14,240,000 | | | Yes |
| 14. Mich. State | 14,182,000 | | | Yes |
| 15. U. Chicago | 12,642,000 | | | Yes |
| 16. Indiana U. | 12,029,000 | | | Yes |
| 17. U. Penn. | 11,329,000 | | | Yes |
| 18. Princeton | 11,267,000 | Yes | | Yes |
| 19. U. Minnesota | 10,745,000 | | | Yes |
| 20. Northwestern | 9,648,000 | | | Yes |
| TOTAL TOP 20 46% of NSF Funds | 366,117,000 | 4 of 4 | 3 of 8 | ALL |

## GEOGRAPHIC DISTRIBUTION OF THE TOP 20 RECIPIENTS OF NSF FUNDS - FY83

**EAST: 6**
**TOTAL $126,168,000**
16% of NSF Total

| | | |
|---|---|---|
| MA: | Harvard | 14,330,000 |
| | MIT | 33,860,000 |
| NJ: | Princeton | 11,267,000 |
| NY: | Columbia | 21,253,000 |
| | Cornell | 34,129,000 |
| PA: | U. Penn | 11,329,000 |

**MIDWEST: 8**
**TOTAL: $113,822,000**
14% of NSF Total

| | | |
|---|---|---|
| IL: | Northwestern | 9,648,000 |
| | U. Chicago | 12,642,000 |
| | U. IL-Urbana | 21,526,000 |
| IN: | Indiana U. | 12,029,000 |
| MI: | Mich. State | 14,182,000 |
| | U. Michigan | 15,368,000 |
| MN: | U. Minnesota | 10,745,000 |
| WI: | U. Wisconsin | 17,682,000 |

**WEST: 6**
**TOTAL: $126,127,000**
16% of NSF Total

| | | |
|---|---|---|
| CA: | U.C.-Berkeley | $19,141,000 |
| | U.C.L.A. | 14,240,000 |
| | U.C.-San Diego | 37,863,000 |
| | CA Inst. Tech | 15,489,000 |
| | Stanford | 21,522,000 |
| WA: | U. Washington | 17,872,000 |

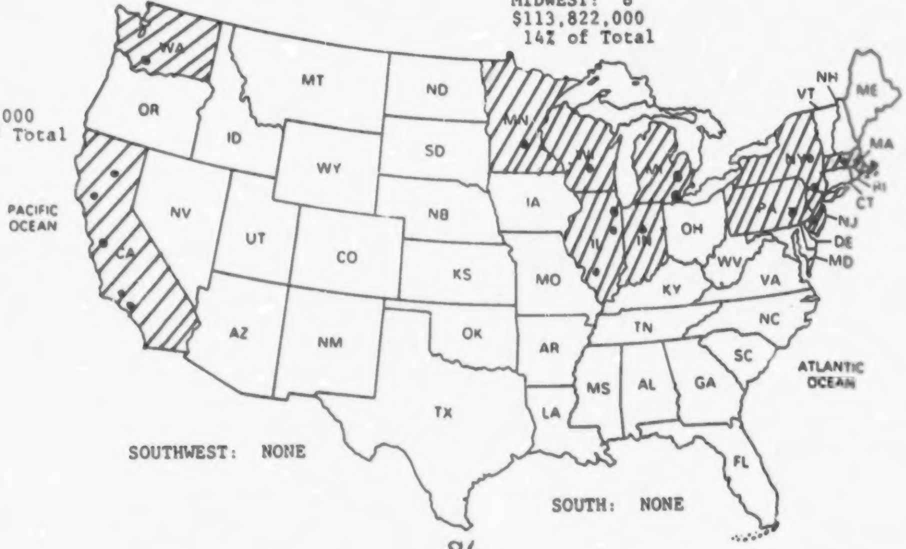
**SOUTH:** NONE

**SOUTHWEST:** NONE

## GEOGRAPHIC DISTRIBUTION OF THE TOP 20 RECIPIENTS OF NSF FUNDS FY83

| | |
|---|---|
| NATIONAL TOTAL | $798,769,000 |
| TOTAL TOP 20 | $688,075,000 |

▨ States in which the Top 20 are located


WA
OR
CA
ID
NV
MT
WY
UT
AZ
CO
NM
ND
SD
NB
KS
OK
TX
MN
IA
MO
AR
LA
WI
IL
MI
IN
OH
KY
TN
MS
AL
GA
FL
SC
NC
VA
WV
PA
NY
VT
NH
ME
MA
RI
CT
NJ
DE
MD
PACIFIC OCEAN
ATLANTIC OCEAN

WEST: 6
$126,127,000
16% of Total

MIDWEST: 8
$113,822,000
14% of Total

EAST: 6
$126,168,00
16% of Tot.

SOUTHWEST: NONE

SOUTH: NONE

86

TABLE B-16. — FEDERAL OBLIGATIONS FOR RESEARCH & DEVELOPMENT TO THE 100 UNIVERSITIES AND COLLEGES RECEIVING THE LARGEST AMOUNTS BY AGENCY: FY [illegible] 1/

CONTINUED

(DOLLARS IN THOUSANDS)

| RANK | INSTITUTIONS (RANKED BY AMOUNT RECEIVED) | TOTAL | USDA | DOC | DOD | ED | DOE | EPA | HHS | INT | NASA | NSF | OTHER* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 51 | INDIANA UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 52 | UNIVERSITY OF MIAMI | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 53 | UNIVERSITY OF VIRGINIA | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 54 | U TEX HLTH SCI CTR DALLAS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 55 | NEW MEXICO STATE UNIV | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 56 | PRINCETON UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 57 | UNIVERSITY OF CONNECTICUT | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 58 | ROCKEFELLER UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 59 | COLORADO STATE UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 60 | SUNY AT STONY BROOK | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
|  | TOTAL 1ST 60 INSTITUTIONS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 61 | TUFTS UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 62 | UNIV OF HAWAII-MANOA | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 63 | UNIV OF CAL IRVINE | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 64 | CUNY MT SINAI SCH OF MED | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 65 | UNIVERSITY OF GEORGIA | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 66 | UNIVERSITY OF NEW MEXICO | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 67 | LOUISIANA STATE UNIV | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 68 | GEORGETOWN UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 69 | N C STATE UNIV AT RALEIGH | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 70 | VA POLYTECH INST & ST U | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
|  | TOTAL 1ST 70 INSTITUTIONS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 71 | EMORY UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 72 | UNIVERSITY OF CINCINNATI | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 73 | VIRGINIA COMMONWLTH UNIV | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 74 | BROWN UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 75 | RUTGERS THE ST UNIV OF NJ | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 76 | UNIV OF MD BALT PROF SCH | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 77 | UNIV OF MASS AT AMHERST | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 78 | UNIVERSITY OF DAYTON | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 79 | GEORGE WASHINGTON UNIV | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 80 | SUNY AT BUFFALO | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
|  | TOTAL 1ST 80 INSTITUTIONS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 81 | UNIV OF MISSOURI COLUMBIA | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 82 | UNIV OF CAL SANTA BARBARA | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 83 | UNIV OF ILL MED CTR CHGO | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 84 | TEMPLE UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 85 | UTAH STATE UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 86 | WASHINGTON STATE UNIV | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 87 | UNIVERSITY OF KANSAS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 88 | WAYNE STATE UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 89 | U TEXAS SYSTEM CANCER CTR | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 90 | U TENNESSEE KNOXVILLE | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
|  | TOTAL 1ST 90 INSTITUTIONS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 91 | U TEX HLTH SCI CTR S ANTO | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 92 | SUNY AT ALBANY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 93 | UNIVERSITY OF KENTUCKY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 94 | FLORIDA STATE UNIVERSITY | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 95 | UNIV OF RHODE ISLAND | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 96 | U TEX HLTH SCI CTR HOUSTN | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 97 | UNIV OF VT & ST AGRIC COL | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 98 | RENSSELAER POLYTECH INST | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 99 | IOWA ST U OF SCI & TECH | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
| 100 | UNIV OF MED & DENT OF N J | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |
|  | TOTAL 1ST 100 INSTITUTIONS | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] | [illegible] |

1/ DOES NOT INCLUDE R&D OBLIGATIONS TO UNIVERSITY-ASSOCIATED FEDERALLY FUNDED RESEARCH AND DEVELOPMENT CENTERS (FFRDC'S). SEE TABLES B-[illegible] AND B-[illegible].

* INCLUDES DOT, AID, HUD, LABOR, AND NRC.

SOURCE: NATIONAL SCIENCE FOUNDATION

# House Task Force Examines R&D Facilities Issue

*The chronic problem of money for university research facilities received a thorough going over May 21-22 before the Science Policy Task Force of the House Science and Technology Committee when a well-selected group of witnesses set forth the views of academe, industry, and government.*

*The hearings were successful for bringing out bedrock positions. Thus, academe repeated that it's sinking and desperately needs help; industry replied that universities manage poorly; the Administration's representative agreed with both, but said that industry and the universities themselves should provide more help—adding, significantly, that federal R&D priorities should be reoriented toward more spending on campus. Following are distillations of the testimony of 3 of the 8 witnesses at the hearings.*

*

**Donald N. Langenberg, Chancellor, University of Illinois at Chicago:** We have just completed an audit of all university buildings . . . . Fifty-six percent of the buildings on the Urbana campus and 44 percent of the total on both campuses are over 50 years old. The total cost to renovate the better buildings and to replace the worst is estimated at just under $600 million . . . . A considerable portion of these are research facilities. In summary, the University of Illinois has an immediate research-facilities deficit conservatively estimated at several hundred million dollars.

In the absence of a cohesive national effort, universities are attempting to address the capital deficit by a variety of means. Debt is mounting in many institutions as they borrow funds, use available bonding authorities, leverage available funds with other private and state funds, and cost-share with other institutions . . . . I believe most already are stretching their imaginations and resources to the prudent limit, and sometimes beyond.

A satisfactory solution lies beyond the capacity of almost all institutions. That broader effort must come from a well-conceived, well-coordinated national program led by the federal government . . . working throught its . . . major research agencies.

*

**Frank B. Sprow, Vice President, Exxon Research and Engineering Company:** Many of us in industry are shocked when we experience the current state of many university laboratories . . . . The estimate of funds [by NSF of $1.3 billion per year needed for new facilities] is not surprising. My own company recently completed construction of a new laboratory . . . to provide state-of-the-art facilities for several hundred scientists. The cost of this facility was over $200 million, corresponding to over $300 per square foot of lab space . . . .

## *Indirect Roulette's Top 20*

*With even the skinflint Reagan Administration having given up attempts at control, indirect costs on federal research grants remain a computational mystery for which there is no explanation but academe's need for all it can get. The biggest provider of such funds is NIH, which, at the request of the House Appropriations Committee, drew up a list of its top 20 university grant recipients and their direct and indirect receipts. The list is included in the text of hearings held in March on the NIH budget for fiscal 1986. Copies are available from the US Government Printing Office or, better yet, your Congressman or Senator. (Ask for House Appropriations Hearings, Department of Labor, HHS, Education and Related Agencies, for 1986, NIH, Parts 4A and 4B.)*

*

| Institution | Direct | Indirect | Total |
|---|---|---|---|
| | (dollars in thousands) | | |
| Johns Hopkins | $49,561 | $27,185 | $76,746 |
| UC San Francisco | 57,126 | 14,521 | 71,647 |
| Harvard | 40,393 | 26,662 | 67,055 |
| Yale | 40,800 | 24,309 | 65,109 |
| U. of Pennsylvania | 33,533 | 18,477 | 52,010 |
| Stanford | 39,851 | 23,856 | 63,707 |
| Columbia | 36,314 | 23,908 | 60,222 |
| U. of Washington | 43,395 | 13,870 | 57,265 |
| UCLA | 40,893 | 14,849 | 55,742 |
| Washington (St. L.) | 32,496 | 15,106 | 47,602 |
| Yeshiva | 26,287 | 21,249 | 47,536 |
| U. of Michigan | 30,265 | 17,061 | 47,326 |
| U. Wisconsin (Mad.) | 33,400 | 12,882 | 46,282 |
| U. of Minnesota | 33,426 | 12,005 | 45,431 |
| Duke | 28,790 | 13,015 | 41,805 |
| UC San Diego | 30,855 | 9,544 | 40,399 |
| U. of Chicago | 23,674 | 14,582 | 38,256 |
| Cornell | 26,045 | 11,672 | 37,717 |
| MIT | 24,250 | 12,685 | 36,935 |
| UC Berkeley | 25,412 | 11,090 | 36,502 |
| Totals | $696,766 | $338,528 | $1,035,294 |

What has often been overlooked in the discussion has been the need for better *systems* for managing, operating, sharing, and stewarding research resources. Management issues have largely been left unaddressed, due perhaps to our highly decentralized system of university research. It is time that we address them because there are abundant opportunities to both increase research output and efficiency . . . .

*(Continued on page 7)*

# . . . Shift R&D Funds to Academe, OSTP Aide Says

*(Continued from page 6)*

The current project grant system . . . has in several ways adversely affected the maintenance of our research infrastructure, particularly in the area of instrumentation. The intense competition . . . has affected the funding allocation decisions of peer review committees, often leading to specific denial of funds for instrumentation . . . . It has led some investigators to defer acquisition of instrumentation in order to use limited funds to preserve scientific and support staff.

An alternative approach that would lend itself to greater utilization of business principles for managing our research resources would be the creation of a new Supplemental Institutional Equipment Grant to encourage the establishment of centralized facilities. Such facilities would be collaboratively managed by the institutions using them. As envisioned here, they would facilitate the acquisition, maintenance, and sharing of instrumentation . . . .

The concept of shared research facilities is already established in the field of physics [and] there are several successful university, industry, and government cooperative arrangements in operation today [in various other fields of research] . . . . Research is becoming so capital-intensive that proven business procedures and techniques must be used to ensure that our investments yield maximum scientific and technological return.

•

**Bernadine Healy, Deputy Director, White House Office of Science and Technology Policy:** . . . the central question . . . is not so much what to do about the present condition of the university research infrastructure. The real question is more fundamental: Is the partnership among industry, government, and the universities functioning in a manner which ensures that the US will maintain a healthy, modern research infrastructure? . . . .

By the late 1970s, the universities warned that unless the government came up with new facilities funding, the research infrastructure was in trouble. Industry was making some contribution, but those were small compared to the benefits they derived from . . . the universities. The universities themselves behaved largely as dependents of the government, abdicating their responsibility for infrastructure and biding their time until federal facilities programs were resumed. And the government . . . attempted not to invest in the research enterprise, but to procure packets of research at the lowest possible price . . . .

Well, what should we do? Simply creating a new, multi-billion dollar facilities program may, over the near term, improve the condition of the infrastructure, but . . . it is equally important that change take place in the attitudes and performance of each of the three partners.

The government must focus on our research expenditures as investing in the research enterprise and not procuring research results. This means bearing the reasonable and necessary costs of the research it sponsors . . . .

. . . universities must assume a far more significant and responsible role in managing the nation's investment in university research . . . . I would like . . . to see a system in which the universities would be reimbursed realistically for facilities and equipment used in federally sponsored research and for the universities to take a leadership role in identifying cost savings associated with research overhead.

As for industry . . . . contributions of state-of-the-art research equipment, and industry-university cooperation in its use and maintanance, is one remedy for many weaknesses in the partnership. Unrestricted donations, as well as donations toward renovation or construction of new facilities, should also be encouraged . . . .

An increased federal commitment to university research is indeed an investment . . . that we probably can't get along without . . . . Yet, of the more than $20 billion [that the federal government spends] on civilian R&D, about $6 billion is invested in university research. This balance may be inappropriate to today's circumstances. Since the budget deficit forces us to select among competing priorities, I would suggest that we continue what we all began several years ago, and redirect civilian R&D funds from lower priority areas, particularly technology development projects, to the highest priority, university-based research.

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## LEGISLATIVE MANDATE OF THE NATIONAL SCIENCE FOUNDATION

The National Science Foundation was established by the National Science Foundation Act of 1950 (Pub. L. No. 81-507; 42 U.S.C. Sections 1861 et seq.). The functions of the Foundation include:

> (e) In exercising the authority and discharging the functions referred to in the foregoing subsections, it shall be an objective of the Foundation to strengthen research and education in the sciences, including independent research by individuals, *throughout the United States, and to avoid undue concentration of such research and education* [emphasis added; 42 U.S.C. Section 1862].

The Conference Report on the act makes it clear that the very structure of the NSF, with a division of authority and responsibility between the Director and the National Science Board, was designed to prevent the "undue concentration of . . . research and education" referred to above:

> Section 5(b) of the conference substitute, relating to the powers and duties of the Director, is the same provision contained in the House amendment except that it has been modified to make it clear that in each instance where the Director takes any final action under Section 10 [relating to scholarships and graduate fellowships] or 11(c) [relating to research contracts] the Board must review and specifically approve the action proposed to be taken. It is the view of the conference committee that this requirement makes more certain than otherwise might be the case that, in conformity with section 3(b) [relating to undue concentration of research and education, now 42 U.S.C. Section 1862(e)], contracts or other arrangements made under section 11(c) for carrying on basic scientific research activities will not be unduly concentrated in a few organizations or institutions or in a limited area of the Nation [1950 *United States Code Congressional Service*, at 2277].

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The New York Times/Marty [illegible]

Dr. Steve A. Orszag, head of supercomputer center, on Princeton campus.

# On Science, Politics and the Path to $200 Million

By RON SUSKIND

On a spring morning in 1981, 11 university scientists gathered around a conference table in Washington to talk about the problems in getting access to large computers.

It was the start of what was to become an intensive, four-year lobbying effort, led in large part by Dr. Kenneth Wilson of Cornell and Dr. Steve A. Orszag of Princeton, to secure Federal funding for supercomputers on college campuses.

The payoff came two weeks ago when the National Science Foundation announced that it would give out $200 million over five years to four universities — Cornell, Princeton, the University of Illinois and the University of California at San Diego — to create four supercomputer centers on their campuses.

Getting the grants, according to Dr. Wilson and Dr. Orszag, involved months of lobbying members of Congress, Federal officials and corporate executives.

What finally turned the tide, they said, was the impact of two unexpected developments a week apart in October 1982.

One involved a report to the National Science Foundation by scientists meeting in Washington a year after that spring session in 1981.

But the full impact of the report was not felt until six months later. A news account about it caused a stir by quoting a sentence from the introductory [illegible] that the United States would lose its lead in supercomputer technology if a national program was not begun.

"The most amazing thing to me was how the media picked it up and a little insignificant group of words could change everything," Dr. Wilson said.

## 'And Everyone Listens'

The other development gave the report even more impact — Dr. Wilson, the leading scientist behind it, won the Nobel Prize in Physics.

"You win a Nobel Prize," Dr. Orszag said, "and everyone listens to everything you say. Ken learned how to put things just right."

Dr. Wilson began traveling around the country — on the road at least one week every month — honing his skills as a public speaker.

Dr. Wilson said he learned an important lesson in public relations: "The substance of it all is too complicated to get across — it's the image that is important. The image of this computer program as the key to our technological leadership is what drives the interplay between people like ourselves and the media and forms a reaction from congressmen."

## Cultivating Lawmakers

Dr. Wilson said he began to cultivate lawmakers, [illegible] Representative [illegible], a Democrat from upstate New York's [illegible] District, which covers part of the Cornell campus.

"[illegible]," Dr. Wilson said. "[illegible]."

Mr. [illegible], who was approached by Dr. Wilson just after his election in 1982, was appointed to the House Committee on Science and Technology [illegible] to the National Science Foundation.

Dr. Wilson [illegible] the committee, and [illegible] at the committee's hearings in February 1983.

Dr. Orszag, meanwhile, was busy meeting with members of the [illegible], with other scientists at symposiums, and with officials who used supercomputers in the Pentagon, the Energy Department and national laboratories.

In 1983 Dr. Orszag left the Massachusetts Institute of Technology to help Princeton build its program in applied and computational mathematics.

## Heading Supercomputer Sites

The two professors were among the most instrumental lobbyists in persuading the Federal Government to approve the grants for supercomputer centers, according to officials of the National Science Foundation. Both are now heading the supercomputer projects at their universities.

Dr. Wilson, 48 years old, won the Nobel Prize for analyzing phenomena involving a seemingly random motion of atomic particles that had puzzled scientists for centuries.

Nowadays, he said, he finds that his cloistered research days are behind him, at least temporarily, and that he is dealing with computer corporations and wearing a tie every day.

"But, as always, his shirttail still hangs out," said his wife, Alison Brown, a leading figure in the supercomputer project at Cornell.

Dr. Orszag, 42, is a professor of applied and computational mathematics. He is credited with developing systems that have advanced the use of Newtonian physics as tools in computerized problem solving.

He is also an entrepreneur who helped found Flow Industries, now a $100 million-a-year company. He also runs a scientific consulting company and has developed theories on investment and arbitrage.

## A Physicist and an Engineer

"Ken is interested in uncovering the laws of the universe," said Dr. Orszag, a precise man with a quick smile. "I'm interested in the practical world of how to apply these laws. That's the difference between us, between the physicist and the engineer."

The two first met in 1973, when they were introduced by a mutual friend. Together they hiked the mountains near Los Alamos, N.M., and talked of their research.

By 1980, Dr. Wilson recalled, he found it impossible to find computers powerful enough to solve his problems in particle physics.

Dr. Orszag, who was still at M.I.T. then, could gain limited access to supercomputers to advance his research. But he often had to use standard machines — "like riding in a horse and buggy while jets are flying overhead."

THE NEW YORK TIMES SATURDAY, MARCH 16, 1985

## DISCUSSION

Mr. **Fuqua**. Thank you very much, Mr. President.

As I gather, the thrust of your statement is that you are really separating basic scientific research from facilities and tend to think, with some reservation, that using peer review to evaluate pure basic research still has some merit even though the system seems to be tilted toward the "haves" versus the "have nots."

Dr. **Silber**. It does indeed have merit in this context, but even there, I think that there should be a very careful scrutiny of the way the peer review process goes to ensure that it is not simply a "old-boy network" in which friends approve the grants of friends, or friends secure those outside advisors who are going to likely be favorable toward the grants that they already know in advance they wish to fund.

But the basic point that I am making is that those institutions that are at the very top in terms of receiving financial aid from the Federal Government, on the one hand, have no hesitation in lobbying the Congress directly when it serves their interest, at the same time that they denounce other institutions when they do so; but they also lobby successfully to the Congress for the funding of those various agencies to which they go for their peer review projects, and they establish in that process a very intimate connection with staff, a good will with the staff, and it follows just like night to day that when the staff get around to setting up those peer reviews, the peer reviews decide to give the money to the various universities that were helpful in getting the funds in the first place from the Congress.

So I am saying that the peer review process itself, even for pure research and basic research, is clouded by the lobbying efforts of the most successful universities in establishing the funds that are then distributed through peer review.

Mr. **Fuqua**. I thought it was an interesting parallel you drew with the NSF supercomputer program, because it appears that there are no qualified universities in the Southeast and South, for that matter, that qualify for that program. Only one went south of the Frost Belt.

Mr. **Silber**. As a matter of fact, as long as you refuse to place a supercomputer in the South, there is not going to be any university in the South that will be competent. If you don't have the facilities and if you don't have the instrumentation—which costs a great deal of money, which requires a huge outlay of capital—then you are never going to be able to attract the scientists who can use that equipment and make it into a productive center, a distinguished center.

But if you were to locate a supercomputer in a university—I don't care which university—if you were to locate a supercomputer there, and all of the facilities and money necessary to sustain it, that university would have no difficulty in recruiting outstanding individuals to run it.

Mr. **Fuqua**. Do you recall any other periods in our history when the question of a balance between scientific expertise and political judgment has been the subject of debate?

Mr. **Silber**. Well, I think we have had—I don't know whether these kinds of issues are ones you have in mind, but I think, with regard to the issue of slavery, there were experts who testified on this, and Congress decided one way or another. There have been issues of abortion in which experts and scientists have been involved.

I must say that in both of these issues, I think the scientific contribution has indeed been limited. I don't think that the issues that—most social issues that have been addressed by the country are so complex that they go well beyond the restraints of any scientist and even beyond the limitations and methodology of science per se if you gathered a group of scientists together.

I think the issue of exploration of space, the Kennedy decision that we would be first to the Moon, for example, was a decision on which you could bring together a group of scientists, but I don't believe that question was a question that could be decided by scientific experts. You had to decide some way on whether it is worthwhile to get to the Moon, and that is not a decision that could be made by scientists. If scientists were to decide what would they rather spend their money on, there might be many projects and places they would rather go than the Moon.

But if you saw this project in the context of the national defense, the exploration of space becomes not only interesting scientifically—for which we might not be willing to pay those billions of dollars—but it becomes absolutely vital to the protection of our country and for our survival as a free Nation.

Now, those political questions simply fuse with the science questions. The point at which the scientific community was absolutely essential was in this area. They had to advise the President on whether you could really get to the Moon. The feasibility of the project of flying to the Moon had to be determined largely by engineers, not by pure scientists, by engineers applying the knowledge that pure scientists or basic scientists had developed.

You take the principles of physics; you take the principles of metallurgy, those developed by chemists, by physicists; you take all the high-powered mathematics; you take all the information available in terms of computers and the control they give us, and decide whether or not that is feasible. That much the scientific and engineering community could do. To decide whether it ought to be done was a matter for the Congress, and I don't know of any single expert on that subject.

Mr. **Fuqua**. Someone suggested that instead of peer review and grantsmanship, which I assume you would think played a role in the peer review process, where people are better at preparing grant proposals than others—some people are better at expressing themselves than others—that maybe it would be better if we had a modified system that asked not necessarily what do you plan to do but what is your track record; what have you done in the past 3 to 5 years in scientific research; how many graduate students have you had? Do you think that would be a help in getting a better distribution of the basic research grants?

Dr. **Silber**. No, I really doubt that it would, because I think you have this chicken-and-egg problem. If the person has never got any research grants, he is not going to have any graduate students, so he doesn't have any record to talk about. If he has had enough success in grantsmanship to be able to support some graduate students, he probably has already learned how to write those things.

Now, with regard to the preparing of grants and the development of grantsmanship, it is amazing that the most talented people seem to acquire this skill very quickly and without very much difficulty. I think that one thing we could do is let them see copies of successful grants, and that is usually the way it happens: An older scientist shows a brand new Ph.D. in science what his grant application looked like, or the young person puts together his grant application and he takes it to an intellectual "Dutch uncle" who looks it over and shows him what is wrong with it.

There were people like John Wheeler at Princeton, who is now at the University of Texas, to whom any number of scientists went for guidance and counsel on how to prepare grants themselves, and such helping hands from well established scientists is essential. It is a kind of master-apprentice relationship that provides for the transfer of connoisseurship, and I think that part is essential.

It might be worthwhile, however, if some anonymity were introduced into the review process, or if there were a more careful recognition of the need by the NSF to grant a certain number of grants to beginners.

But it is fair to make the judgment that has been made that you could waste a great deal of money simply by gambling one grant on every beginner. I have seen an amazing amount of research that was stupid on its face and incapable of coming to any successful conclusion that was, nevertheless, funded.

For example, I recall reading an article in which a psychologist had made a study of the dirtiest words in the English language, and in his instrument, he never bothered to ask those who were advised to rank order the 50 words that were given whether they even knew the meaning of the words that they were rank ordering. And so it was clearly a flawed piece of research right from the start. But it was done by a scientist with a very fine reputation, and it had been funded by granting agencies.

Now, those kinds of mistakes don't bother me. I think, if you want a vigorous scientific community, you might as well write off a certain percentage of your research activity as a waste, and you can have a field day, a la Senator Proxmire, with some of the ridiculous things that happen. And the ones that he picks are really ridiculous. Nine times out of ten, he picks a corker.

But that goes with the human condition. That is not the sound basis for criticizing peer review. The sound basis for its criticism is to recognize that you really do have a club. You have a closed club that takes care of one another. They carry the concept of courtesy to one's fellow club members to a very high level, and that needs to be broken up. There has to be a way that those of us who busted our noses on the window glass outside the candy store can get into the candy store.

Mr. FUQUA. Mr. Lujan.

Mr. LUJAN. Thank you, Mr. Chairman.

You don't have to answer, but I don't know why I would want to know which is the dirtiest of all 50 words. [Laughter.]

I don't know what particular use I would have of that unless I wanted to cuss somebody out, and I guess we could do that very well.

Dr. **Silber** If you wanted to write a successful novel, it would help to know. [Laughter.]

Mr. **Lujan**. Well, all right. Very good.

I have in front of me the list of the top 10 and the top 20 that NSF funds. The disturbing part of it is that if you made that kind of a list for almost all these other agencies, the same universities would be there. And what you say about the economic impacts—I know, in my own home State, I don't know what that portion of the State where both Sandia and Los Alamos are located would be without those laboratories. As a matter of fact, I remember seeing somewhere that in those counties that are pretty close to Los Alamos, the five counties there or something, Los Alamos alone is 20 percent of the entire income for the whole area. So the economic impact of research is tremendous.

It is no accident that up in the Northeast, all of the—I don't know what they call it. It is not called Silicon Valley; that is California.

Dr. **Silber**. It is called Route 128.

Mr. **Lujan**. Well, whatever, yes. It is because of those universities; the same thing with Silicon Valley. It just has seemed very unfair to me, and I agree with all of the things that you are saying.

Now, from time to time, I have written letters, and I am sure other Members of Congress have written letters, about the bias that is shown in the distribution of these funds. I was just writing some of the ways that we might be able to change that. Rotation? I don't know how the people are picked, but perhaps rotation or a percentage of those reviewers of grants could be from some of the small universities.

You mentioned line items for the national laboratories, formulas by which we run our agricultural research. You mentioned anonymity. Do you have any other ideas as to how we might be able to break this hold?

Dr. **Silber**. I think one way that you can move in this direction is, for example, to recognize that group of liberal arts colleges that came forward recently, recognizing that although they received almost no support from the NSF, nevertheless, they prepare about 7 percent of the young men and women who go on into research in the sciences. Now, their need for funds could be designated as a category that would be open to liberal arts colleges that want to maintain distinguished undergraduate science instruction programs, and that would be one way of coming up with that.

With regard to graduate research and to high-level basic research, I think, rather than jiggling around with the process, I think it is a matter of putting the NSF on notice, particularly Mr. Blanpied, who suggested using peer review as a club to beat wayward institutions over the head, to inform him that that had better not happen and that the Congress will scrutinize from time to time the peer review process in the NSF, but then, having put them on notice, leave them alone to manage the NSF program and continue to respond on an ad hoc basis to the legitimate claims that are made for facilities development.

Now, when facilities are developed, the peer review process becomes open suddenly to places it couldn't do it before. Boston University ranks somewhere around fortieth in total Federal grants, and we will move, as a result of our development of the new science center, substantially higher. We will be more competitive under the peer review system, on the assumption that vengeance is not played out for us. If we get an even hand in the future, we will do better simply because we will have far greater scientists at work at Boston University than we could have had without these new facilities.

So, I think that the soundest way of opening up that list of universities competing under peer review is to perpetuate this process of locating your basic centers and your facilities and your instruments in universities that have promise, that can make very good use of them, but that have not yet developed into that top rank. That is the way I think that it is most effectively spelled out.

I think if you start directly tampering with the peer review system, that it is really too subtle for adjustment or correction by courts of law or by the Congress. It is a matter of connoisseurship. The serving on one of those panels is a position of trust. It is a position that calls for moral integrity, intellectual integrity, and very high-level competence, and I don't think you can legislate any of those things.

So, I would not advocate the introduction of rules of the Congress to direct the NSF or any other agency in how to engage in their peer review process, but I would suggest to them that we are concerned to extend, in granting funds to the NSF, the scientific and technological strength of this Nation in a broader way than it has been extended so far, and see if that counsel and advice is not enough.

Mr. LUJAN. Are facilities kind of the cornerstone, as you see it, to begin to build this excellent university?

Dr. SILBER. Facilities and equipment. The supercomputor is a beautiful example. It is not a facility, but it is a piece of equipment. And if you've got $50 million, you can have one, and if you don't, you're out of business. And it is hard to come by $50 million over a short period of time.

Mr. LUJAN. Thank you, Mr. Chairman.

Mr. FUQUA. Mr. Walgren.

Mr. WALGREN. Thank you, Mr. Chairman.

I certainly would like to underscore the sympathetic chord that your testimony has struck in me and, I am sure, other Members. I guess I am really amazed that we have come to the point where a witness comes before the Congress with real apprehensions that they are possibly going to suffer adverse consequences, and their institution, out of what the reaction of others might be to their testimony. I think that is a pretty sad state of affairs.

I wonder, Mr. Chairman, whether it might be helpful if we could submit for the record the article by Dr. Atkinson and Dr. Blanpied that President Silber has cited. It is a very interesting article, and I think it would be important to underscore in there the directness of the suggestion that anybody who violated this code of operations was supposed to be effectively sanctioned.

My purpose, really, in submitting that article is that I don't think it will stand scrutiny for 10 seconds. The sequence of development of thoughts in that article struck me as extremely flimsy, and I think it would be interesting to set it off against the testimony that you have given, because I think it will perhaps give some life to what is driving the other side of this argument.

I think it also should be underscored how destructive is the thought of denying those who would receive some funding from some other source, other than peer review, denying them access then to apply for research funding under the peer review system. It strikes me that either you believe in the peer review system or you don't, and you don't walk away from the most meritorious research proposal and then fund something that is a waste of money because of your judgment about the background of the entity that is suggesting that the meritorious research be done.

I think the fact that that could even be suggested shows how the dynamics of this question are very powerful, and leading logical people to be illogical, and perhaps even with the potential of doing damage to others that clearly is not appropriate.

So, if I could make that as a suggestion, Mr. Chairman, I would like to do that.

Mr. FUQUA. Yes; we will make that a part of the record.

[Material follows:]

# PEER REVIEW AND THE PUBLIC INTEREST

*Richard C. Atkinson and*
*William A. Blanpied*

**PROLOGUE:** *The unique contract between science and government that has existed in the United States since the end of World War II rests on the assumption that science must remain autonomous but that the public interest will best be served if scientists play a decisive role in determining how public funds are spent to support scientific research. The notion that the government can delegate authority over the distribution of public money to the beneficiaries of that largess is remarkable. That it can do so without the intrusion of corrupting influences and without threatening the autonomy of science is due largely to the principle of peer review.*

*Here, Richard C. Atkinson, former director of the National Science Foundation, and William A. Blanpied, currently international studies specialist at the National Science Foundation, warn that the peer review principle is in jeopardy. By using "pork barrel" tactics to obtain funds for research facilities, bypassing the traditional process of consultation and peer review, a number of universities threaten to reduce science to just another special interest lobby. Defense of the peer review principle is essential, the authors argue, to restore a healthy relationship between science and government and to ensure the continued effectiveness of our national scientific research effort.*

*Richard C. Atkinson received his Ph.D. in philosophy from Indiana University in 1955 and served on the faculty of Stanford University for 20 years. An experimental psychologist and applied mathematician, his research has been concerned with problems of human memory and cognition. Atkinson was director of the National Science Foundation for five years and has been chancellor of the University of California at San Diego since 1980.*

*William A. Blanpied received his Ph.D. in experimental nuclear physics from Princeton University in 1959 and has taught at Case Western Reserve, Yale, and Harvard universities. He was cofounder (with Gerald Holton) and first editor of the journal* Science, Technology and Human Values, *and head of public sector programs at the American Association for the Advancement of Science before joining the National Science Foundation in 1976.*

The present relationship between science and government in the United States is remarkable.[1] It would have taken a particularly reckless prophet to predict, even 50 years ago, that the scientific community could convince a succession of administrations and Congresses that support for basic research in universities is not only a legitimate role for government, but a responsibility. Today most U.S. scientists probably do not find anything out of the ordinary in a policy that delegates to the scientific community decisions about the expenditure of funds appropriated by Congress for scientific research. But 40 years ago influential people in the White House and Congress were asking whether the public interest could be served if decisions about research priorities were left in the hands of scientists.[2] At the same time a few conservative leaders within the scientific community were expressing skepticism that any system of safeguards could guarantee that federal support for science would not lead to federal control and the inevitable corruption of fundamental scientific values.[3]

We now have reason to believe that these concerns should not be dismissed as alarmist. For example, in 1983 a Senate floor amendment to a supplemental appropriations bill for the Department of Education earmarked $15 million for construction of a space and marine science building at the University of New Hampshire, a step that heralded what was soon to be labeled scientific pork barreling. This is a tactic used to obtain funds for research facilities from Congress through last-minute floor amendments to government agency funding bills, thus circumventing project evaluation by the broader scientific community, by the agency, or by a designated congressional committee.[4]

The Office of Management and Budget's (OMB) characterization of scientists as "the quintessential special interest group"[5] is certainly exaggerated. Yet pork barreling is not in the overall interests of science or society and may reinforce the view, apparent in some quarters, that scientists are in fact just another special interest lobby. Certainly pork barreling and OMB's apparent contempt for the scientific community's pleas for increased research support are indicative of strains in the relationship between science and government. We will argue in this paper that these strains are due, in part, to both parties' partial abrogation of the explicit contract they concluded in the aftermath of World War II. The burden of our argument is that both science and society will be better served if the scientific community recognizes that it must assume a strong, coherent negotiating stance in its relationship with government, as it did 40 years ago. Defense of the peer review principle is essential to the achievement of that stance.

## II

By 1943 a consensus was emerging within the scientific community, the then Bureau of the Budget, and Congress that the close working relations established between science and government during the wartime emergency should be sustained. Yet questions about the character of that relationship remained. For example, would the public interest be served by a policy of establishing closer links between science and government? Who should define the public interest? How would it be guarded? What would constitute an intrusion on scientific autonomy? Finally, since the principle (dating from the seventeenth century) that scientists alone are qualified to determine research priorities—the peer review principle—was conceded to be central to the preservation of autonomy, how should the relevant peer group be selected and what scope of authority should government delegate to it?

In May 1950 the creation of the National Science Foundation ended five years of negotiation between the scientific community and the government. Although many questions were still incompletely resolved, it was assumed, at least by the key parties in government, that future disagreements could be settled by good-faith negotiations.[6] Viewed from that perspective, the apparent belief among much of the scientific community that government support for research is a virtual entitlement abrogates the post-World War II contract.

The post-World War II agreement between science and government was—and is— a political contract negotiated in the political arena according to political rules by a broad spectrum of scientists who exhibited considerable skill in the process. They succeeded in large measure because they were able to elevate issues important to science to the status of important national issues. One such issue was direct federal support for research and science education in universities. But the five-year debate on that issue was linked with, and conditioned by, negotiations over the relationship between science and the military,[7] civilian versus military control of atomic energy,[8] and the terms under which the scientific community could accept direct support from government and provide policy advice in return.[9]

The unique feature of that contract was the assumption that science would best serve the public interest if scientists, as private citizens, retained decisive influence over how public funds were spent to support scientific activities. The integrity of peer review was regarded as essential in making that part of the contract workable. Erosion of the principle of peer review by tampering with the normal appropriations process not only undermines quality control, but threatens to reduce the scientific community, in the eyes of Congress and the White House, to "... just another set of hands being held out for a share of the Federal pie."[10]

## III

Although peer review is usually understood as a recently developed process for allocating government research funds to individuals working in nongovernment institutions, the principle actually emerged in the seventeenth century. By the end of the eighteenth century, there existed a federation of self-governing learned societies dedicated to the disciplined search for useful knowledge that included, for example, the Royal Society of London (chartered in 1660),[11] the American Philosophical Society (1743),[12] and the Asiatic Society of Bengal (1784).[13] The proceedings of these societies provided the principal mode of communication among their members. Significantly, proceedings also served as a means for new societies to establish their credentials among their peers.[14] The integrity of the proceedings of each society was ensured, in turn, by an editor who relied on an advisory board—in effect, a peer review panel—to review prior to publication all members' contributions. That precedent was established in 1664 when the Council of the Royal Society licensed the publication of a regular proceedings and took upon itself what was to become, within a century, an editorial control role.[15]

As peer review was emerging as a means of ensuring quality control, the learned societies were also attempting to define their relationship with the larger society. Almost from the outset a dichotomy existed between what Stephen Toulmin has referred to as the Newtonian ideal of science as a worthwhile end in itself and the Baconian ideal of science as a means of achieving social benefits. That is, the societies, while asserting their autonomy, also compromised it by seeking official sanction for their activities as well as continuing reassurance about the social value of the activities. Resolution of the dichotomy between autonomy and accountability has required that science continually reexamine and negotiate its relationship with government, no matter how reluctantly it has done so, and no matter how often it has denied doing so.

***Thomas Jefferson understood that while the interests of science and government may overlap, they are rarely congruent.***

The idea that scientific rationality could provide the basis for an enlightened political system was central to the thinking of the founders of the American Republic. In particular, as Don K. Price argues, they subscribed to the conviction that "truth," as exemplified by science, would provide an effective counterbalance to the potential excess of political power.[16]

The convergence of science and government during the 50 years following American independence was epitomized by Thomas Jefferson, who simultaneously served as president of the American Philosophical Society and president of the United States. Yet, as his initiative in connection with the 1804–06 Lewis and Clark Expedition suggests, Jefferson understood that while the interests of science and government may overlap, they are rarely congruent. In 1803 he convinced Congress to appropriate $2,500 " . . . for the purpose of extending the external commerce of the United States. . . ."[17] To ensure that the expedition would serve the needs of government, Jefferson groomed his personal secretary, Captain Meriwether Lewis, as its leader. Mindful of his desire that the expedition also serve science, Jefferson established what was, in effect, a peer advisory committee by dispatching Lewis to Philadelphia to receive instruction from members of the American Philosophical Society on celestial observations, on the collection of botanical and zoological specimens, and the study of the customs of American Indians.

In applying what can be called peer review or peer monitoring to a scientific project that had significant policy implications, Jefferson extended the principle beyond its original quality-control function. In addition, the peer review principle came to serve as a buffer against external, nonscientific interests, and as a means for forging an alliance between scientific interests and other interests—in this case, commerce. In fact, Jefferson had already established a closely related precedent when, as the nation's first patent officer, he turned for advice to an expert panel from the University of Pennsylvania, and in that way extended peer review to an external government advisory function.[18]

The importance of peer review for the scientific community—both to ensure quality control and to define an internal governance framework for science—became increasingly significant from the late nineteenth century onward as science emerged as a full-time profession centered in universities and industrial laboratories.[19] The internal governance function is evident, for example, in the establishment of quasi-official institutions such as the National Academy of Sciences in 1863, and the National Research Council as an adjunct to the National Academy of Sciences in 1916, and also official bodies such as the National Advisory Committee for Aeronautics (which was established in 1915 and was to serve in World War II as the model for the Office of Scientific Research and Development),[20] or the ill-fated Science Advisory Board experiment of the early New Deal era.[21] In all these cases government sought to institutionalize science policy advice, and in all cases scientists were able to maintain control over the conditions for providing that advice. These pre–World War II institutions had a mixed record of success in fulfilling the objective of providing useful policy advice to government. However, the autonomy maintained through peer control remained intact and was respected by government.

## IV

Viewed against this historical background, the postwar science-government contract that attempted to bring science into the political system while at the same time preserving its autonomy was a truly daring innovation. No arrangement of comparable importance exists in other countries, and there was never any assurance that the peer review system in the United States would remain vigorous enough to protect science from the corrosive influences of politics. It is in that context that scientific pork barreling must be examined. For nothing in the contract required one party to defend the values of the other in the event that the party should default, as scientists and administrators in at least 15 universities have done in recent years.

A February 1985 report of the National Science Board's ad hoc Committee on Excellence in Science and Engineering documents these largely successful attempts by universities to obtain authorizations and appropriations for facilities (valued at over $100 million) by taking their claims directly to Congress. Often the universities retained professional lobbying firms to assist them.[22] These incidents include the following:

- A total of $13.9 million in fiscal years 1984 and 1985 appropriations for the Department of Energy for construction of a vitreous state laboratory at Catholic University.
- A total of $8.0 million in the Department of Energy's appropriations for the same two years for construction of a chemical research laboratory at Columbia University.
- A $7 million appropriation added to the Department of Energy's fiscal year 1985 budget to permit Florida State University to construct a supercomputer center and acquire instrumentation.
- A $4.5 million add-on to the National Institutes of Health's fiscal year 1985 appropriation to facilitate the development of a cancer research center at West Virginia University.

- An $800,000 add-on to the appropriation for the National Oceanic and Atmospheric Administration's fiscal year 1984 budget to enhance the University of North Carolina's undersea research program.

Of the 15 pork barrel incidents documented in the National Science Board committee report, five were add-ons to Department of Energy appropriations bills, four to appropriations bills for units within the Department of Health and Human Services, and one to a National Oceanic and Atmospheric Administration appropriations bill. The remaining five incidents involved agencies that are not major supporters of basic research; namely, the Department of Education (four incidents) and the Economic Development Administration (one incident). All 15 actions involved construction of facilities for the conduct of research rather than funds for research itself. But the committee report suggests that a next logical step would be to use pork barreling to seek funds for such research support.

***Pork barrel tactics violate the understanding that available resources are to be allocated in the best interests of science—and the public. . . .***

As more than one critic has suggested, the point at issue is not whether meritorious research will be carried out in facilities obtained through pork barrel tactics.[23] Rather, those tactics violate the understanding that available resources are to be allocated in the best overall interests of science—and the public—rather than in the interests of individual claimants, no matter how qualified or deserving they may be. At another level pork barreling underlines the dependence of research universities on federal largess and suggests that the potential for the corruption of scientific values by access to political power that was feared by conservative scientific critics of the post-World War II contract is a legitimate concern.

Yet thus far the peer review principle has preserved considerably more autonomy for science in the United States than anywhere else in the world. Translated into practice, the central tenet of the earlier, implicit agreement between science and government—that truth should be kept separate from power—has meant that the U.S. government has provided support to universities by means of research grants to individuals distributed on a competitive basis according to criteria and procedures largely controlled by the scientific community. One disadvantage of this system is that the uncertainties and instabilities inherent in two- or three-year funding cycles make long-term planning by universities difficult.

The situation in Western Europe, Japan, and particularly Eastern Europe is quite different.[24] In other countries universities receive stable, baseline operating support from the central and sometimes state or provincial governments and, with a very few exceptions, are firmly controlled by government. Additionally, almost all national governments except the United States provide baseline support to a parallel basic research system separate from the university system. (These nonuniversity research systems differ from U.S. government laboratories, which are either managed by universities or consortia of universities or managed directly by a federal agency for specific, mission-oriented purposes.) Many governments also provide some research funds on a competitive basis to scientists working within the university and national research systems. However, the magnitude of the support available for this purpose is small relative to the continuing baseline support.

American scientists clearly pay a price for trying to preserve the indirect, pluralistic support system based on peer review. But they also acquire benefits. Most other countries provide stable support to universities. But they do not recognize universities as the principal sites for the conduct of basic research. Informed observers in both the United States and Western Europe (as well as some from other countries, including the People's Republic of China[25]) agree that whereas the system in effect elsewhere is more stable, it is also much less effective in encouraging competition among the most innovative ideas, particularly those of young scientists. University scientists, as government employees, also cannot claim the same degree of autonomy they can in the United States.

## V

Conceivably those scientists who have convinced their universities to compromise their values so blatantly would not have done so had they believed that they could obtain resources in some more legitimate fashion—or if they had believed that they would be effectively censured by their colleagues for their tactics. Have those scientists lost confidence in peer review? How fair and effective is the process as presently implemented?

Peer review operates most directly and successfully when experts from the same discipline or related sets of disciplines make priority rankings of research proposals within established programs. Similar procedures are frequently followed in competitions for special types of facilities, although in these cases the reviews are usually more extensive, require approvals at more levels within an agency, and may involve criteria, such as geographical balance, in addition to scientific and technical merit.

Broadly analogous implementation procedures are followed at the project level at many of the principal agencies that support basic research in universities—e.g., the National Institutes of Health, the National Science Foundation, and those units with the National Aeronautics and Space Administration, the Department of Defense, and the Department of Energy that support external research and fund special research facilities. The most significant procedural differences relate to the discretion of agency program officers with respect to the judgments of external peer reviewers and panelists. At the National Institutes of Health, for example, priority rankings of review panels (known as study groups) are binding. At the National Science Foundation, where these judgments are advisory, program officers are at liberty to make a case to the agency to modify external peer rankings for good and sufficient reason.

Questions about the fairness of peer review are almost inevitable, particularly since, in most cases, programs do not have sufficient funds to support all proposals that are judged as meritorious by external peers. However, independent assessments have concluded that at the project level peer review generally operates to distribute funds on the basis of merit in the context of criteria established by the agencies themselves—usually in consultation with external advisory panels.[26] But the issue of the effectiveness of peer review as opposed to its fairness is more germane to the science–government relationship.

For example, should priorities within a program be established strictly on the basis of intrinsic merit, or should added weight be given to meritorious projects that promise rapid advances in understanding in a particularly critical area—even at the expense of equally meritorious work in other less dynamic areas? In other words, should peer review operate only to evaluate merit or should it also help establish priorities? Can it or should it be effective in changing the direction of a program, in allocating resources among programs within agencies, or in changing the scientific directions of the agencies themselves? These questions are significant because they challenge the assumption that peer review is the best possible way to allocate resources in the best overall interests of both science and society.

Peer review operates less directly and less effectively at the program than at the project level. Each government agency negotiates the details of its annual budget first with the Office of Management and Budget and then with a set of congressional committees. External advisory bodies can often help an agency define appropriate and feasible program directions. They can and are effective in marshaling the support of the scientific community to save programs threatened with extinction by the Office of Management and Budget. But with one notable exception, those bodies have almost never had to make priority judgments that are almost certain to distress respected colleagues and institutions. That exception is the Department of Energy's High Energy Physics Advisory Panel, which formerly served as an external advisory body to the Atomic Energy Commisson and the Energy Research and Development Administration. Members of that advisory panel recognized in the early 1960s that support would not be forthcoming for the construction and operation of new particle accelerators required for frontier research unless older facilities (many still capable of useful research) were shut down. The fact that this advisory panel has been able to reach and generally enforce consensus on priorities to optimize the overall health of the field may be one important reason for the ability of high-energy physics to continue to garner substantial financial resources from government.

Quasi-official peer advisory panels have demonstrated the potential to deal more effectively with the painful decisions inherent in the resource allocation problem—at least on the disciplinary or program level—than most official panels, with the notable exception of the High Energy Physics Advisory Panel. Beginning in 1962, the Committee on Science and Public Policy of the National Research Council, with the support and encouragement of the National Science Foundation and other agencies, has convened successions of panels for particular scientific disciplines to make recommendations concerning the most fruitful long-term research directions.[27] Within the last five years these panels have begun to face up to the priorities question, which they had largely avoided. For example, the centerpiece of the 1982 report on astronomy and astrophysics was a listing by priority of facilities required to exploit opportunities for the balance of the century.[28] The highest priority was assigned to construction of the Very Long Baseline Array radio telescope, and that priority is reflected in the National Science Foundation's long-range-planning document for fiscal years 1986–90. Currently, the Committee to Survey Opportunities in the Chemical Sciences has reportedly reached consensus on three priority areas in the field.[29] A comparison of skillfully staged previews of this committee's report with the report of another National Research Council committee on chemistry published 20 years ago, which emphatically refused to refer to priorities, suggests that the scientific community's attitude toward its responsibility for making difficult decisions in the long-range interests of science may be changing.[30]

The problem of establishing priorities across rather than within disciplines has yet to be clearly faced, although attempts in that direction have been made. The Office of Science and Technology Policy, which from 1976 to 1982 was required by law to prepare for Congress a *Five-Year Outlook* on science and technology, agreed with the National Science Foundation that the National Research Council should be asked to convene representatives from a range of scientific fields to examine their own and related disciplines and to identify research areas of particular importance both to science and to the resolution of important national issues.[31] More recently, the Committee on Science, Engineering and Public Policy has, at the request of the president's science adviser, prepared a series of annual research briefings that address these issues.[32] Although these devices have been useful for information exchange and for helping the separate disciplines sort out their own priorities, there is no evidence that they have had any appreciable effect in determining resource allocation across disciplines or among agency programs.

**The rapid growth of the defense-related components of the R&D budget since 1981 has occurred with little effective input from the scientific community.**

## VI

There is the larger problem of whether peer review can or should operate at an even higher level of aggregation to allocate resources *among* federal R&D agencies, or even help determine the overall size of the federal R&D budget. Because policy considerations other than scientific and technical excellence and promise are involved at this level, it is tempting to conclude that scientific peer review has no applicability whatsoever. Recent experience appears to bear out this conclusion. For example, the rapid growth of the defense-related components of the R&D budget since 1981—coupled with the decline, in real dollars, of the civilian components—has occurred with little effective input from the scientific community.[33] On the contrary, until well into the 1960s, government actually expected scientists to provide substantial advice not only about levels and allocations of research support, but also about other important science-related policy matters. Until 1957, however, official spokesmen for science rebuffed government's repeated offers to give the scientific community a voice in resolving such issues.

In February 1951 the newly organized National Science Board (the legislated policymaking body of the National Science Foundation) rejected the Bureau of the Budget's request that it play a major role in planning and coordinating federal research allocations; for the next five years the National Science Foundation rejected similar appeals by the bureau.[34] But the psychological crisis occasioned by the Soviet Union's launching of Sputnik in 1957 finally brought science, for about a decade, firmly into the political system very much as the Bureau of the Budget had envisioned and on terms that largely preserved scientific autonomy. The President's Science Advisory

Committee, created late that year by President Eisenhower, was a prestigious scientific peer group expected to provide independent advice at the highest levels of government on issues important to both science and society, including R&D budget allocations.[35] Significantly, government implicitly accepted the claims of science to a particular level of autonomous disinterest by conceding that (unlike the Council of Economic Advisers, for example) scientific competence alone, rather than competence and approved political leanings, should serve as a basis for membership on the President's Science Advisory Committee.

No doubt the waning and eventual extinction of this peer committee during the Vietnam era was due in large measure to the fact that its members often assumed positions that were inconsistent with the policies of the Johnson and, later, the Nixon, administrations.[36] But as the fate of many individuals in the White House during those years attests, it is unlikely that preoccupation with its own survival would have spared the committee. On the contrary, such a course might have damaged science's reputation for disinterest and integrity. In any event, the lesson that some scientists seem to have learned from the demise of the President's Science Advisory Committee—that science can be badly burned if it approaches government too closely—may be the wrong one. Rather, the continued waning of science's influence with government during the past decade—as suggested by the current imbalance between the military and civilian components of the federal R&D budget or the impasse that has apparently developed on the issue of open scientific communication—suggests a different lesson; namely, that science should discipline itself to speak out strongly and coherently on important policy issues even though it may, on occasion, suffer setbacks as a consequence.

## VII

Science and government both assumed in the late 1940s that a coherent set of strategies was required to bring scientific resources and capabilities to bear on important national issues. Science policy was implicitly defined as the sum total of those strategies. During its first decade the President's Science Advisory Committee came close to defining and implementing such a national science policy. Viewed in that context, support for university research was regarded as being in the public interest because such support would amplify resources critical to the nation.

Today many scientists tend to regard research support as an end in itself and entanglement with other issues as either unnecessary, dangerous, or both. The National Science Board seems to have taken such an attitude when it rebuffed the Bureau of the Budget in the early 1950s. But in doing so it also abdicated a good deal of the political authority it might have had to negotiate on behalf of the scientific community. At any rate, the assumption that research is a sacrosanct activity that government must continue to support adequately has lulled much of the scientific community into a state of political apathy and has allowed government to treat science as if it were, in fact, just another special interest. Even the informed public, if it comes to regard science in that way, will have difficulty understanding why scientists become upset because some of their colleagues claim a piece of the federal budget through the same pork barrel tactics that other special interest groups have traditionally used.

Scientific pork barreling probably cannot be completely halted unless the scientific community severely disciplines those who engage in it, or unless its root cause—the deterioration of university science facilities—receives national attention. Precedent suggests that the viability of the universities can, in fact, become a national issue. During the 1960s the President's Science Advisory Committee was able to convince the Kennedy and Johnson administrations that it was in the national interest to increase the number of first-rate research universities in the country, and to award "centers-of-excellence" grants competitively to do so.[37] But that occurred only because science was in a strong position to negotiate from a perspective of national rather than parochial interests.

It is, of course, unlikely that the resources required to conduct all potentially meritorious research, or to plan, construct, and operate all special scientific facilities that could be used to good advantage, will be forthcoming from the government in the near future. Recognition of that state of affairs has, as already noted, led several scientific disciplines to face the difficult problem of forging a consensus about their priorities. By doing so they have acquired the strength and cohesion required to negotiate with individual government agencies for the resources they need, at least for the most essential elements of their programs. While this trend is promising, it also represents a piecemeal approach to the problem of allocating resources. It does not address the problem of the overall size of the federal R&D budget nor its distribution among agencies and programs. Nor does it address the problem of maintaining the country's broad scientific infrastructure. At its extreme the disciplinary approach concedes, in effect, that the scientific community cannot have any significant influence on the overall R&D budget, and reluctantly blesses efforts of the separate disciplines to press their independent cases for marginal budget increments or even larger pieces of a fixed pie.

***Scientific pork barreling cannot be halted unless the scientific community disciplines those who engage in it or the deterioration of university science facilities receives national attention.***

Could peer review processes be extended to yield a broad consensus not only about research priorities within disciplines but also about priorities across disciplines and priorities for both moderately expensive research facilities and very expensive programs? Could the scientific community arrive at such a broad consensus even if some institutions and some disciplines were to suffer as a consequence? Could a sufficiently strong consensus be reached about the fairness and effectiveness of procedures for establishing priorities so that individuals and institutions that attempted to circumvent those decisions could be effectively censured by the rest of the community—for example, by being threatened with denial of subsequent support by official peer panels?

Two decided advantages might accrue to science if it could, at the very least, move in these directions. First, by presenting a more united front, the community could lay claim to a stronger voice not only in allocating existing resources but also with respect to other important science-related policy issues—including the overall size and distribution of the federal R&D budget. Second, an effective demonstration that science is not just another special interest lobby would legitimize the importance of preserving scientific autonomy not only for science but for society. And it would give to the scientific community a good deal of the political and moral authority required to negotiate issues of genuine national importance.

Several recent cases illustrate the effectiveness with which high-level, semi-independent advisory bodies that enjoy the confidence of the scientific community can elevate issues of interest to science to the status of national issues. The Defense Science Board, by questioning whether the use of export control regulations to restrict international scientific communication is in the national interest, expanded the constituency with a stake in that issue and probably forestalled even heavier-handed attempts to limit such communication than are now being suggested. Doing so ensured that the issue would be resolved at the highest levels of government and with the participation of the scientific community.[38] The National Science Board, by invoking a little-used authority granted it by the National Science Foundation Act of 1950, established a distinguished, broadly representative Commission on Precollege Education in Mathematics, Science and Technology, and thereby allied the scientific community with other groups concerned with the deterioration of precollege education.[39] As a final example, even the moderate success of university scientists from agriculture-related fields in establishing a competitive grant program occurred because those scientists allied themselves with other groups, including scientists in other fields who correctly saw the issue as important to ensure the viability of peer review, and with private interests that have a stake in the quality of basic research in agriculture.[40]

Of course these examples can also be taken as illustrations of the limits of science's current influence with government and the need for it to develop stronger political alliances. The full objectives of science have not been attained in any of the cases cited above. However, the fact that the scientific community has managed to speak with a strong, coherent, and largely disinterested voice and to gain substantial public attention in these matters should not be overlooked.

Can the scientific community speak with the same strength, coherence, and relative disinterest on the single issue that it is most qualified to address? A strong science and technology infrastructure and maintenance of the viability of the universities as the basis for that infrastructure are at least as important to the nation now as they were 40 years ago. But establishing such an infrastructure requires more than just adequate support for research and research facilities. It also requires that science preserve a large measure of autonomy for detailed decisions about overall directions for research. However, as we have argued here, scientific autonomy has always been negotiated within a political framework—with the expectation that its protection serves the public interest. Perhaps, as some critics suggest, a new science-government contract is needed to suit current realities. Perhaps modification of the present contract will be sufficient. In neither case is the outcome likely to be in the best long-term interests of either science or the public unless science is able to deal with government from a position of strength and to recognize that support for research is linked with other important policy problems.

The central issue to be addressed is not whether the scientific community should enter the political arena; science is in that arena whether or not scientists speak there effectively on its behalf. Rather, the issue is whether the community will enter that arena from a position of strength and thus have a reasonable chance of affecting policy decisions, or whether it will decline to play the political game and be forced to live with decisions that it has, at best, a small voice in determining.

The paradoxical claim that society will obtain maximum benefits from science if scientists are allowed to pursue their work free from intervention is as old as Bacon, and it is unlikely that challenges to that assumption will cease in the near future. If so, the best safeguard that science has against unwarranted intrusions is its long-standing reputation for integrity—a reputation based on public confidence in the ability of scientists to govern themselves in the best interests of the larger society. Rigorous application of peer review as a means for self-governance has been a critical factor in maintaining science's autonomy in the changing circumstances of the past three centuries. A continued defense of that principle and a continued demonstration of its viability, even at the risk of considerable distress to some members of the community, is the best course available to science to serve both its own best interests and the best interests of the larger society. ■

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**NOTES:**

1. The opinions of the authors are their own and do not necessarily reflect the policies of their institutions.
2. Daniel J. Kevles, "The National Science Foundation and the Debate Over Postwar Research Policy, 1942–1945: a Political Interpretation of Science—the Endless Frontier," *Isis* 68 (1977): 5–26. J. Merton England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945–57* (Washington, D.C., National Science Foundation, 1982), 9–107.
3. England, *Patron for Pure Science*. One of the principal skeptics was Frank Jewett, president of the National Academy of Sciences, whose skepticism regarding the dangers of overly intimate links with government dates at least from World War I; (cf. A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities to 1940* (Cambridge: The Belknap Press of Harvard University Press, 1957) 324 and 337.
4. *Science* 222 (1983): 592; *Science* 223 (1984): 27; *Science* 226 (1984): 519; *Congressional Record* (Sept. 25, 1984): E 4002–4003. For a political analysis of the origins of the pork barrel phenomenon see Philip M. Smith and Albert H. Teich, "University Research Facilties and Pork Barrel Politics" (in press).
5. In *Science and Government Report* Daniel Greenberg attributed this remark to an unidentified OMB official; George Keyworth II, President Reagan's science adviser, quoted it in an address delivered at the AAAS Colloquium on R&D in the Federal Budget, April 3, 1985.
6. William D. Carey, "Science and Public Policy," *Science, Technology and Human Values* 10, no. 1 (Winter 1985): 7–16.
7. cf. Daniel J. Kevles, "Scientists, the Military, and the Control of Postwar Defense Research: The Case of the Research Board for National Security, 1944–46," *Technology and Culture* 16 (Jan. 1975): 20–47.
8. Alice Kimball Smith, *A Peril and a Hope: The Scientists' Movement in America, 1945–47* (Chicago: University of Chicago Press, 1965).
9. Vannevar Bush, *Science—The Endless Frontier*. First issued as a report to the president of the United States, July 5, 1945. Reprinted in 1980 by the National Science Foundation, Washington, D.C.; Detlev W. Bronk, "Science Advice in the White House: The Genesis of the President's Science Advisers and the National Science Foundation," in *Science Advice to the President*, ed. William T. Golden (New York: Pergamon Press, 1980) 245–256.
10. George A. Keyworth II, *C&EN News* 63, no. 5 (April 15, 1985): 5.
11. Thomas Sprat, *History of the Royal Society* (London, 1667). Reprinted by Routledge and Kegan Paul, Ltd. (London), 1958, ed. Jackson Cope and Harold Jones.
12. Alexandra Oleson and Sanborn C. Brown, eds., *The Pursuit of Knowledge in the Early American Republic* (Baltimore: Johns Hopkins University Press, 1976), xvi.
13. William A. Blanpied, "Notes for a Study of the Early Scientific Work of the Asiatic Society of Bengal," *Japanese Studies in the History of Science* 12 (1973): 122–44.

14. A. Hunter Dupree, "The National Pattern of American Learned Societies, 1769–1863," in *Pursuit of Knowledge*, ed. Oleson and Brown, 21–32.

15. Harriet Zuckerman and Robert K. Merton, "Patterns of Evaluation in Science: Institutionalization, Structure and Functions of the Referee System," *Minerva* IX, no. 1 (Jan. 1971): 68–69.

16. Don K. Price, *America's Unwritten Constitution: Science, Religion, and Political Responsibility* (Baton Rouge: Louisiana State University Press, 1983).

17. Dupree, *Science in the Federal Government*, 31–36.

18. Dupree, *Science in the Federal Government*, 26–28.

19. Dupree, *Science in the Federal Government*, 120–48, 289–325.

20. Dupree, *Science in the Federal Government*, 283–87.

21. Dupree, *Science in the Federal Government*, 350–58. Daniel J. Kevles, *The Physicists* (New York: Alfred A. Knopf, 1978), 252–58.

22. "Report of the NSB Committee on Excellence in Science and Engineering" (NSB–85–50. Available from the National Science Board, National Science Foundation, Washington, D.C. 20550).

23. Smith and Teich, *University Research Facilities*, Norman Hackerman and Warren D. Niederhauser, *C&EN News* 63, no. 16 (April 22, 1985): 31.

24. cf. "Research in Europe and the United States," in *The Five-Year Outlook on Science and Technology, 1981*, vol. 1 (Washington, D.C.: National Science Foundation, 1981), 255–84.

25. A small competitive grant system has been established within the research system of the Academia Sinica. (cf. William A. Blanpied, *Science, Technology and Human Values* 9, no. 2 (Spring 1984): 67–72.) More recently, the state Science and Technology Commission of the People's Republic of China has announced its intention to establish a national science foundation to support research on a competitive, peer-reviewed basis, and Chinese officials visited the National Science Foundation in Washington during the spring of 1985 for intensive discussions on details.

26. cf. S. Cole, J.R. Cole, and G.A. Simon, *Science* 214 (1981): 881–86.

27. William W. Lowrance, *Science* 197 (1977): 1254–60.

28. Astronomy Survey Committee of the National Academy of Sciences (George Field, chairman), *Astronomy and Astrophysics for the 1980s* (Washington, D.C.: National Academy Press, 1982).

29. National Research Council, *Report of the Committee to Survey Opportunities in the Chemical Sciences* (Washington, D.C.: National Academy Press, due 1985). For a preliminary indication of the tenor of the committee's report see Committee on Science, Engineering and Public Policy, "Report of the Research Briefing Panel on Selected Opportunities in Chemistry," *Research Briefings 1983* (Washington, D.C.: National Academy Press, 1983) 65–92.

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31. Three successive editions of the *Five-Year Outlook* were prepared by the National Science Foundation under the guidance of the Office of Science and Technology Policy and with substantial input from the National Research Council and the American Association for the Advancement of Science. These were published by the National Science Foundation in May 1980, January 1982, and September 1983, respectively. The legislated requirement for a *Five-Year Outlook* was eliminated by the omnibus Reports Elimination Act of 1982.

32. Committee on Science, Engineering and Public Policy, *Research Briefings 1983* and *Research Briefings 1984* (Washington, D.C.: National Academy Press, 1983, 1984).

33. American Association for the Advancement of Science, *Research and Development in the Federal Budget*, volumes for fiscal years 1982 through 1986 (Washington, D.C.: American Association for the Advancement of Science).

34. England, *Patron for Pure Science*, 131–210. Milton Lomask, *A Minor Miracle: An Informal History of the National Science Foundation* (Washington, D.C.: National Science Foundation, 1976), 91–110.

35. Golden, *Science Advice to the President*.

36. David Z. Beckler, "The Precarious Life of Science in the White House," in *Science and its Public: The Changing Relationship*, ed. Gerald Holton and William A. Blanpied (Boston: D. Reidel Publishing Co., 1976), 114–34.

37. cf. President's Science Advisory Committee, *The Universities and the Federal Government* ("The Seaborg Report"), Nov. 1960.

38. cf. National Academy of Sciences, *Scientific Communication and National Security* (Washington, D.C.: National Academy Press, 1982).

39. National Science Board Commission on Precollege Education in Mathematics, Science and Technology, *Educating Americans for the 21st Century* (Washington, D.C.: National Science Foundation, 1983).

40. cf. Don F. Hadwiger, *The Politics of Agricultural Research* (Lincoln, Neb.: University of Nebraska Press, 1982).

Dr. **Silber**. Might I just respond to one point?

Mr. **Walgren**. Yes.

Dr. **Silber**. The fact that I think I am reasonable in recognizing this as a threat may be supported—I don't know this is the case—but I think the committee might be well advised to consider if they had invited other college presidents to testify who were supportive of the direct approach to the Congress, and if they had any others that had accepted.

I have looked down the list of witnesses, and I know that there are many college presidents who very much agree, at least by their practice if not by their words, and I know of many others who agree by both their words and their practice with the point of view I have taken, but I don't see them on the list to testify, and I don't think they are coming forward for any other reason than that they share my apprehension about what that might do to the future of their competitiveness in the peer review program. I think it is a very genuine concern.

Mr. **Walgren**. I wonder if there is any suggestion we could make for the peer review system. I have no background in it, so I am really not qualified to comment on it. But it does strike me that the recognition of the important questions and the appreciation of the techniques of research are not limited simply to those who are most prominent in their field, and, in fact, one could argue that the whole liberal arts training is designed to enable somebody to appreciate, if they then pursue a scientific survey of some kind, to be able to appreciate the kinds of judgments that would be most appropriate in selecting one research proposal over and above another.

In fact, it would strike me that it would be very easy to come up with some kind of suggestion as to what an effective peer review panel would look like. I do get concerned that the taking to oneself of the best scientific judgment then becomes an absolute cloak for whatever institutional friendships and things like that which a person automatically gathers during his lifetime, or future aspirations even to be associated with a future university, or something to that effect.

Would it really be that hard to sit down and figure out what a truly independent but scientifically sensitive peer review panel would look like?

Dr. **Silber**. I think it would not only be difficult; I think it would be impossible. You have a difficulty that you are taking in your own washing. If you don't take scientific achievement seriously—that is, the achievement of a scientist in the community of scientists— you don't have any place to start. It is not good enough that his momma thinks he is a scientist or his daddy thinks he is a scientist; it is important that the scientists and the scientific community think he is a scientist.

And so you can start off by going to people like John Wheeler and other truly distinguished scientists and say, "Give me the names of 10 people who should serve on panels, and these are the areas in which they are qualified." And you go to other scientists of great renown and great accomplishment, such outstanding achievement that there can be no question about their connoisseurship and their competence as scientists, and you ask them for these lists, and you have got to work out of such lists.

I don't think there is any way that somebody trained in the liberal arts is going to be able to come up with a specified rule about whom you get. The fairness of that scientific review is going to depend on the integrity and the scientific quality of the individuals who are on it.

What you can do is be sure that there is not too much jiggering by the staff. The staff can write proposals in such ways that limit the people who can apply. For example, at Boston University, we compete overseas in Germany for grants to provide educational services to the Department of the Army. Well, they asked us to provide an MBA program, but they put us in competition sometimes with an institution that runs a noncertified, a nonrecognized MBA program. Well, any damn fool can run a noncertified MBA program cheaper than you can run one that is certified. So, whoever writes up the rules for the competition can certainly affect the outcome.

I think people with just normal common sense can do a lot of good to review that. A distinguished scientist could probably do more. Picking somebody to make that kind of review of the way in which staff work is done from time to time might be useful. But, basically, that is not the failing.

I think the reason why you get the concentration is that you started those centers in universities that had access to lots of dollars, and once they got the high-quality faculty there, it is no mystery that they continue to get it.

I don't think that is the worst of it at all. The worst part of it is, if you want to create many centers of academic excellence and scientific excellence, and if you want to increase the number of industrial and technological and economically vital centers in this country, you have to put out the capital into these new areas so that they can begin to attract the people who can work successfully in peer review.

Mr. **Walgren**. I see. So, you don't have any real reservation about how the peer review system itself is operating; it is simply that there are areas that it should operate in and areas that you feel it constrains, if followed solely, and extended to facilities and that sort of thing?

Dr. **Silber**. Oh, I have real concerns when a member of the staff—it is hard to call him merely a member of the staff—when a director like Blanpied suggests that they use peer review panels for the purpose of punishments. That means that the very integrity of the peer review process is being compromised by a person who is in a position to compromise it.

Now, I think that is totally destructive of the peer review system, and I think that that kind of thing has to be corrected. But the peer review system, run to the best of a decent man's ability, run to the best of the ability of a highly qualified person who is the director of the NSF or the director of the NIH or the director of any one of those other agencies and his staff, assuming that they are qualified individuals themselves, and assuming that they are persons of integrity, I don't think you are going to find any weaknesses in it that don't go with the territory of mankind.

They are going to occasionally sin; they are going to occasionally be unintelligent; they are going to make some mistakes. But there is not going to be any way that the Congress is going to improve on it by trying to legislate how you keep them from making mistakes, because there isn't any way.

You are just going to have to resign yourself to the fact that if you have a Supreme Court of nine finite individuals, some of the decisions are going to be better than others; and if you have an NSF that is organized by reasonably well qualified people, they occasionally are going to make mistakes, and I wouldn't worry about that.

What I would worry about is when somebody deliberately puts his thumb on the scale. It is when a trade association of universities publicly denounces the direct approach to Congress that you know that the group of "have" schools has become so bloated and self-confident and arrogant that they are now showing overweening pride and beginning to affect the fairness of the process itself, and it is time to put an end to that. But you don't put an end to that by trying to destroy the peer review system, which is probably the best device we have for the determination or the selection between individual projects that are submitted for Federal funding.

Mr. **Walgren**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you very much, President Silber. We appreciate your being here this morning.

Dr. **Silber**. Thank you.

[Answers to questions asked of Dr. Silber follow:]

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COMMITTEE ON SCIENCE
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Office of the President

Boston University
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June 27, 1985

The Honorable Don Fuqua
Chairman
Committee on Science and Technology
Room 2269
Rayburn House Office Building
Washington, D.C. 20515

Dear Chairman Fuqua:

I should like to thank you and your colleagues for affording me the opportunity to present my views at some length at yesterday's hearing of the Congressional Science Policy Task Force.

It has occurred to me that, in responding to a question from Representative Walgren about peer review, I addressed my answer exclusively to the issue of how peer review operates in the assessment of individual scientific research projects. My answer would have been more complete had I also discussed the issue of peer review as it applies to facilities and to decisions concerning the location of facilities.

I remain convinced that peer review is without doubt the best way to handle individual research projects -- always assuming, of course, that the system operates as it was intended to operate, free of bias, prejudice and conflict of interest.

In evaluating proposals to build new facilities, however, the case is entirely different. For example, if someone who proposed to build a new building held a design competition among ten architects, how could one find experts to evaluate submissions who were both unbiased and competent? While it may make excellent sense for scientists, all of whom already have secured employment, to assess scientific projects, architects are not best qualified to decide which design best meets the needs and tastes of the owner. The owner is best qualified. Moreover, architects can always use more work; hence any architect serving on a review panel would be reviewing the work of a competitor or of a firm with which he might collaborate. The conflict of interest is built-in and inevitable.

The same thing is true with regard to the choice of a contractor to build a new project. No one would ask contractors to serve on a panel to award a job to one of their competitors: here again the owner, who takes all of his interests into account, is the best judge.

In a more general way, the identical problem arises with regard to deciding where to place a major facility. There is no way to empanel a group competent to make an "expert" judgment, because every state and every community would have a competing interest. The ferocious struggle that is already developing over the location of the SCC is an instructive example of this. The people of the United States are the true owners and beneficiaries of such installations and their elected representatives -- the Congress -- are best qualified to decide on those locations which most effectively and equitably meet the needs and interests of the entire country.

In summary, it is obvious that when the "consumers" are the people of the United States -- for example, in the case of major facilities for scientific and technological research and training -- the only competent judges are the people themselves acting through their elected representatives. When the general welfare is at stake, the American people are the only relevant "peers."

Thank you again for inviting me to testify before the Task Force. Your leadership in tackling the important issue of "Science in the Political Process" is deeply appreciated. I wish you and your colleagues well in your deliberations.

Your sincerely,

John R. Silber

John R. Silber

## QUESTIONS AND ANSWERS FOR THE RECORD

### Dr. John R. Silber

1. **Have there, to your knowledge, been comparable cases in other fields, such as law, medicine, defense policy, social security, and welfare policy, where the balance between professional judgment and political judgment has been at issue?**

   Under our system of government, cases where the balance between professional judgment and political judgment has been at issue are not merely unavoidable, they are healthy. Democracy requires that the people, speaking through their elected representatives, have the last word on how public resources shall be used. Public debate is enlivened and the issues more thoroughly canvassed when professionals who can bring their special knowledge and experience to bear take part. But the ultimate authority must remain with the representatives of the people, who are accountable to the people. The clearest indication of how deeply this principle is embedded in the structure of our government is found in the Consttution itself, which provides for the control of the professional military establishment by the civilian legislative and executive branches of government.

   As might be expected, American history is filled with cases in which tension between professional and political judgments has played a major role. If, for example, the President and the Congress had been excessively deferential to their more cautious legal advisers, Abraham Lincoln would not have issued the Emancipation Proclamation, Franklin D. Roosevelt and the Congress of his time would never have passed much of the New Deal's legislative program, and Lyndon B. Johnson and the Congress of his time would never have passed the Civil Rights Act of 1964, particularly its public accommodation provisions. John F. Kennedy's decision to direct the nation's space program towards a landing on the moon was essentially a political decision, made in the face of serious misgivings among members of the scientific community. And if the American Medical Association had had its way, there would of course be no Medicare program for the nation's elderly citizens.

2. **The recent cases of attempts to obtain science facilities for individual universities through the political process are not the only instances where conflict between political and scientific judgments have taken place. We have also seen, for example, attempts by the Congress to establish new, mission-oriented institutes at the National Instututes of Health (arthritis, nursing) which the medical research community opposed. Do you see any generally applicable lessons that can be drawn from all of these cases?**

   The lesson that I draw from cases where political and scientific judgments have been or are in conflict is this: the political branches of the Federal Government -- the Executive and the Congress -- must retain ultimate decision-making authority in matters of public policy and in the allocation of national resources. Retention of this authority does not rule out the widest possible consultation with professionals whose areas of expertise are implicated in public policy decisions. Indeed, the wise exercise of such decision-making authority demands such consultation. Nevertheless, the specialization required of professionals in various fields can produce a kind of "tunnel vision" that sometimes makes it difficult for those professionals to see where the greater good of our society lies. The AMA's opposition to Medicare is a classic example,. Politically accountable institutions like the Executive and Congress are far more likely to appreciate how that greater good is to be secured than any group of professionals, no matter how intelligent.

3. **One of the traditional rules of the political world, arising from the concept of representational government, is that legislators have a duty to look out for the interests of their constituents. This a particularly strong tradition in our American system where we have no members elected "at large" or on the basis of total party votes, but all represent specific geographic areas and the people living within that area. Should we expect legislators in such a system to exempt certain parts of the Federal Government's activities, be it science, or defense, or the arts, or any other field, from such political interest or influence?**

   It would be a mistake to exempt any part of the Federal Government's activities -- whether defense or science or the arts or any other field -- from political interest or influence. The only reasonable grounds for doing so would be if it could be shown that political interest or influence is a bad thing, and that in consequence the public interest would be better served if decisions were made by some other means,. Since our system of government is a democracy, this would mean that democratic processes would be curtailed or set aside in favor of some other method of brokering competing demands and interests. But no one, in my opinion, has yet proposed a workable system of government that is superior to democracy for allocating public goods and services.

   It should be emphasized that this does not amount to an argument that Congress in infallible. Political decisions in a democracy are not always right. The important point is that those decisions represent the will of the electorate -- and if they do not, the representatives who made the decisions are accountable to the electorate in a way that no group of professionals or experts is. So long as we remain a democracy, it is inevitable that political interests or influence will be an essential part of the process by which we govern ourselves. While the results in each case are not always to everyone's liking, Winston Churchill's aphorism remains true: democracy is the worst form of government, except for all the others.

4. **If certain aspects of federal activity, such as science, are exempted from political determination, what happens to the concepts of political accountability for the individual legislator?**

   If certain aspects of federal activity, like science, were exempted from political determination, the political accountability of individual legislators in matters affecting science would disappear. Such an arrangement might be more comfortable for some legislators, but it would be harmful not only to the nation but to the scientific community. Legislators ought to be accountable to the people who elected them; and scientists, to the extent that they are supported by public funds, ought to be accountable for what they do with those funds to the legislators who provide them. Any other arrangement would be incompatible with the fundamental principles of our democracy.

5. **Based on your observations and knowledge, do state legislatures generally reject the practice of seeking university facilities through the political process at that level, or are such legislative provisions concerning science projects and facilities widely used at the state level?**

In my experience, state legislatures generally embrace the practice of seeking university facilities through the political process. State legislatures have been for more than a century deeply involved in the provision of facilities for state-supported universities, and nowadays the more enlightened state legislatures strive to provide facilities support to independent universities as well. Furthermore, there certainly has been no restraint shown by state universities in seeking these funds, whether the facilities were needed or not.

6. **In the event we continue to see the practice of lobbying for research facilities or other attempts to make judgments about science within the political process at about the level of the past 3 to 4 years, what, if any, will the long-term effects be, in your opinion? Is there a threshold level where this practice has serious adverse effects, in your view?**

If lobbying for research facilities continues at the level of the past three or four years, the result will be slowly to strengthen and expand the nation's research infrastructure. That level of support will not begin to meet the enormous need of the nation's universities for new or revitalized research facilities. The nation needs a broadly-based, geographically equitable facilities development program to supplement the individual efforts of those especially energetic institutions that lobby Congress directly for research facilities.

I do not believe there is a threshold level where the practice of lobbying for research facilities would have serious adverse effects. Certainly it has not had such effects to date. As a matter of fact, an increase in such lobbying activity might help to alert both the Congress and the public to the magnitude of the national need for university research facilities.

7. **Would it make any sense to establish a cost limit for science projects and facilities below which legislative specifications should not be made, but above which statutory specifications would be appropriate?**

It is important to distinguish between support for research projects and support for research facilities. Obviously, Congress should not attempt to assume the role of the National Science Foundation in the evaluation of proposals for individual research projects. On the other hand, given the factors involved in decisions to locate federally-supported research facilities, Congress should retain the authority to make such decisions itself, regardless of the amount of money involved. Between these two relatively clear jurisdictional areas there are cases in which the distinction is not clear-cut. The dividing line to be drawn through this gray area may vary in particular cases. In my judgment, the drawing of that line is an appropriate task for a politically responsive and accountable body like Congress.

Mr. **Fuqua**. The next witness will be Dr. Robert M. Rosenzweig, the President of the American Association of Universities.

## STATEMENT OF DR. ROBERT M. ROSENZWEIG, PRESIDENT, ASSOCIATION OF AMERICAN UNIVERSITIES, WASHINGTON, DC

Dr. **Rosenzweig**. Good morning, Mr. Chairman.

I work for a trade association of bloated, overweening, major research universities, and I welcome the opportunity you have given me to appear here this morning to engage in some public handwringing. [Laughter.]

I wish that I could discuss with you the full range of important and fascinating questions included in your study agenda under the heading of Science in the Political Process. I fear, however, that the limits on your time and my knowledge combine to make that impractical.

Instead, I want to discuss with you an extremely important question of science policy, one that has taken on a new and special urgency in recent years. In its broadest and most useful form, the question is: What role should the Congress play in making decisions that affect the conduct of scientific and technological research?

You will note that this is not the related question that is so frequently asked, namely, "What is the proper role of experts and specialists in making policy that has important scientific content?" That question has an ancient and honorable historical lineage and has been the object of a body of literature that has grown larger as the importance of science and technology has grown larger.

The committee will necessarily confront the role of the expert in a variety of forms as it proceeds in this study. For now, though, I want to consider with you the role of the nonexpert—the Member of Congress—in making policies that will profoundly affect what science is done, where it will be done, and who will do it.

Let me start, then, by suggesting that questions about the role of Congress should really be understood to mean the roles of Congress. As science and technology have come to touch ever larger areas of modern life, they have evoked demands for public attention that, in a democracy, quickly become the concern of the public's elected representatives.

The Congress can and often does play a number of different roles, each of them of potentially great import. For example, the Congress frequently must deal with regulatory issues of great significance to the conduct of science. To mention but a few that have arisen in recent years, there have been major debates over the regulation of recombinant DNA research, the use of human subjects in research, the use of animals in research, the propriety of fetal research, a variety of environmental issues, and the wisdom of controlling scientific communication in the interest of national security through the Export Control Act.

In addition to these regulatory activities, and sometimes overlapping them, the Congress has frequently played an active oversight and early-warning role. In recent years, there have been useful hearings on a variety of ethical issues arising from genetic engineering and the growing connections between universities and industry. And, of course, the extensive study by this committee, of which this is a part, is another example.

More conventionally, the Congress affects science by its regular authorizing and appropriating activities, and increasingly through its actions on tax policy, antitrust policy, and patent and copyright policy.

I don't intend here an exhaustive catalog of the important ways in which the Congress, by what it does or does not do, affects the conduct of science. I think the point should be clear, though, that the health of science in the United States is intimately connected with the wisdom of a large number and wide range of decisions made in this legislative body.

It is important to understand, especially so for members of the academic and scientific communities, that the Congress is so deeply involved in science policy because many issues of science policy touch on important values and interests of the citizenry, and because they are frequently causes of conflict.

Issues of that description often find their way into the political arena in a democratic society. That is not something to apologize for; it is something to take pride in, even though the processes of resolution are often messy and in some respects perhaps less satisfactory than some hypothetical ideal.

Having said that, it is also necessary to say that not all issues fitting that description are best dealt with through political instrumentalities, even though they could be. On occasion political leaders, like judges, refrain from involving themselves in issues for a variety of reasons, including the judgment that other means are clearly preferable, and to preempt those means would produce less desirable results and do a disservice to the public.

The task of the Congress with respect to its role in shaping science policy is to decide where to be active and where to exercise restraint; where its distinctive ways of reaching agreement will promote the public's interest in science and technology and where they will not.

Since the Congress necessarily plays many roles, depending on the area of policy under consideration, this challenge is not likely to be met by a single position based on a single over-arching principle. What is called for is the development of a set of expectations about congressional behavior based on thoughtful consideration of the requirements of politics, the requirements of science and technology, and the nature of the decisions to be made.

At this point I must make an essential digression. Universities also have responsibilities and a role to play here. The questions must not be directed solely to the Congress.

If our research system is to continue to thrive and set the standards for research excellence, the task of universities is to gain a deeper appreciation of and respect for the role of the Congress in shaping science policy. No less than the Members of the Congress, universities and their representatives need to exercise judgment and restraint as they present their individual and collective needs to their congressional representatives.

Universities must be sensitive to the reactive nature of the Congress and must respect the necessary and legitimate responsibility of Members to respond to constituent needs. The vulnerabilities of congressional processes should not be exploited for institutional gain if the price of doing so is to weaken further the collective esteem for established and proven processes of allocating national resources for research.

If the Congress needs to fashion a set of expectations about its behavior in the science policy arena, so, too, do individual institutions and their representatives. Self-restraint on both sides will help to prevent harsh economic realities and the resulting politics of scarcity from seriously weakening the foundations of our unique research enterprise.

Some in the Congress and elsewhere have wondered why the AAU and other voluntary organizations do not impose sanctions on members who decide to make direct appeals to the Congress. From personal experience, I can assure you that there are real and important limits on the ability of voluntary associations to control the behavior of their individual members. They simply lack the means to do so without engaging in self-destructive behavior.

That is why the mutual understanding and self-restraint of the Congress and the university community are essential. It is, in addition, a further example of the extraordinary difficulty of designing and operating conspiracies in this society.

The notion that sitting in front of a congressional committee and expressing one's opinion could, through some process among other universities and the executive branch, result in punishment of the institution that the individual represents is so foreign to my understanding of the way the American system of decisionmaking operates and the way the scientific community operates and the way the Congress and the scientific bureaucracy operate that it is utterly, utterly preposterous.

What may be surprising to the committee as it considers this matter is how little attention has been given to the role of Congress in science policy. In sharp contrast to the rich literature examining the role of expert knowledge, I would submit that this committee will have great difficulty in finding guidance from systematic, empirical, or normative analysis of Congress' role in this important domain.

In the last year, we have learned just how unfortunate that inattention has been. As this committee well knows, we have been engaged in a noisy controversy about one aspect of the congressional role, that having to do with the allocation of funds for scientific facilities.

The controversy involves a number of instances in which the Congress has made appropriations for the construction of research facilities at particular universities without either a competitive application process or a professional review prior to approval. The practice appears to have grown in the last 2 years.

Some, including those who have benefited from it, or hope to, say that it is a perfectly legitimate, indeed time-honored, way of doing business. They point to a number of instances in the past in which particular decisions can be traced to the influence of a powerful congressional patron, and they profess to wonder why there is such a fuss simply because what was once done behind the scenes has now become more visible.

Others, of whom I am one, say that this reading of the record, in fact, distorts it, that a fairer reading of the record will show truly remarkable congressional self-restraint in dealing with decisions on scientific facilities, and that actions of the last 2 or 3 years mark a change in behavior which, unchecked, will be inimical to the public's interest in supporting high-quality science.

It has been, until now, an unsatisfactory debate. History has provided no authoritative guide to past practices because no reliable history exists. More fundamentally, the debate has gone on as if it had no connection to the general question of what role the Congress can and must play in forming science policy if the system is to operate properly and, conversely, what actions the Congress should refrain from taking for that same reason.

Consequently, we have seen a hopeless muddle over the difference between the right to take a particular action and the wisdom of doing so. Self-interest has flown under the banner of the first amendment right of citizens to petition the Congress and the corresponding power of the Congress to respond as it chooses; cynicism about universities and science has grown in the Congress and in the press; and cynicism about the Congress has grown in the press, in the universities, and among scientists. I don't think "unsatisfactory" is too strong a word.

And unsatisfactory it will remain unless we can erect a strong and defensible framework for building a thoughtful and useful role for the Congress in science policy, a role that is consistent with the history and traditions of our political system, that meets the legitimate political needs of Members of Congress and defends them against illegitimate demands, and that helps sustain the highest quality scientific and technological work of which our people are capable.

This committee has an opportunity to make an important contribution toward that end. I would like to spend the few minutes remaining to me here by making some suggestions about the contours of that role.

Some propositions seems to me relatively easy and noncontroversial. There is little remaining argument over the proposition that the Federal Government must be centrally involved in the support of fundamental research and training for research in science and technology over a wide range of disciplines. That being so, committees of Congress will be regularly involved in science policy through the usual activities of authorization and appropriation.

Nor is there dispute any longer over the legitimacy of congressional concern for regulatory issues in science. We are long past the time when it could be argued that the imperatives of science and technology sweep aside all other social values and community interests. Environmental and safety concerns, as well as ethical considerations in the uses of new knowledge, are widely seen as legitimate issues for deliberation in the Congress.

If legitimacy of a congressional role is not at issue in these areas, the wisdom of any particular action, or of the need to take action, very likely will be in every instance. I take that not as a denial of congressional responsibility, but as reassuring evidence that the Congress will not be left to discharge its responsibility unchecked by public scrutiny and debate.

I suppose that one could say that the controversy of the last 2 years also involves an area in which the legitimacy of congressional action is not at issue. Certainly, it is true that in other policy domains in which money is distributed, active involvement by Members in who gets how much is the norm. That involvement is not limited to an interest in setting distribution formulas, but frequently reaches directly to decisions about the location of individual projects.

In general, Members of Congress have not been involved in decisions about scientific projects to nearly that degree. Decisions about which scientific projects should be funded have been made almost entirely without direct congressional involvement, and almost always only after competent professional review of the merits of the work to be done.

The record with respect to facilities, as opposed to projects, is more mixed, but it is mixed in a reasonably orderly way that suggests the existence of at least tacit understandings about what is appropriate congressional action and what is not.

The Committee on Science, Engineering, and Public Policy of the National Academy of Sciences, in its recent report, "The Outlook for Science and Technology 1985," illuminates that order by classifying four kinds of facilities projects:

First, national facilities, intended to serve a national, often international, research community; for example, the Fermi National Accelerator Laboratory in Illinois.

Second, university-based research facilities. A new or renovated chemistry or engineering building is an example.

Third, regional facilities usually based at a university. The report cites the Triangle Universities Nuclear Laboratory in Durham, NC, as one example.

And, fourth, technology centers. These are usually located at, or affiliated with, universities that are tied to local or regional economies; for example, the Basic Industry Research Institute at Northwestern University.

Items 1 and 2 on that list can be seen as end points on a continuum. Items 3 and 4 lie somewhere in between. At one end of the continuum lie the large national facilities, which because of their very size have significant economic benefits for the communities in which they are located.

The decisions on where to put those facilities have usually followed heated political struggles, and no doubt they always will. Almost without exception, though, the battle has been waged among competitors that have been chosen from a larger group, using criteria of scientific quality as judged by those who are qualified to make such a judgment.

At the other end of the continuum lie the many decisions about campus facilities for the conduct of programs of research. These were the meat and potatoes of the Government's Facilities Support Program of the 1960's, and the disappearance of those programs in the late 1960's is one important explanation of the pressures that Members of Congress are now feeling.

There was and is no sense in which these could be called national facilities, and in most cases they were not even really regional facilities. Instead, they were essential elements in the capacity of individual institutions to do good research. It was expected that the research done in them would very largely be competitively funded research projects for which faculty would apply and whose quality would be judged by other scientists.

Indeed, one criterion of success in winning facilities funds was the judgment that the research to be housed in the building was likely to be of high enough quality to win such awards. Parenthetically, Mr. Chairman, where the notion arose that facilities programs operated by the Federal Government have not been peer reviewed is a mystery to me. It is simply flatly contradictory to the facts.

What marks the middle two categories is their connection to local or regional economic development. The criteria for locating facilities that are thought to be important to plans for economic development are bound to give weight to considerations other than scientific quality. Economic goods are political prizes and will be fought for as such.

However, that still leaves unanswered the question, What are the rules of battle? That has become a critically important question, because what we have witnessed in the last 2 years is a blurring of the reasonably clear categories that the Academy report described.

Part of that blurring is, it should be said, tactical in nature. The Columbia University chemistry building, for example, which was authorized by floor amendment to the DOE authorization 2 years ago, was described as the National Center for Chemical Research because, I would guess, a stronger case could be made for congressional action on a national facility than on a merely very distinguished chemistry research program of a single university.

But in larger part, the blurring represents a recent and quite widespread belief that science and technology must be the centerpieces of modern economic development. As more States and localities come to believe in that relationship, they will turn to their universities to be leaders of their development programs and to their representatives in Congress to provide the wherewithal for university participation, just as they have long done for flood control, rural electrification, hydroelectric power, and other large capital projects essential to local economic well-being.

Those demands are real; they are not at all frivolous; and they need to be attended to. However, if they are not dealt with by the Congress in a thoughtful way, the consequences to our research system will be severe. The effects of investing our scarce resources in other than the best research will be measured in terms of diminished productivity, decreased competitiveness, and opportunities lost.

Over time, a general weakening of the research enterprise could well result. This, of course, would be precisely the opposite outcome intended by present attempts to strengthen our economic competitiveness and national security.

Some projects, presented to the Congress wrapped in the rubric of economic development, can hardly be justified by the stimulus they will give to local economies, whatever their intrinsic merits might be. Other projects presented to the Congress as national exemplar or national demonstration projects seem able to demonstrate only the political manipulation of congressional authorization and appropriation processes.

Just a few years ago such projects probably would have been wrapped in the rubric of energy independence or the need to protect the environment. Who can predict the project rationale that surely will precede local economic development? I urge the Congress to maintain a commitment to the long-term welfare of our research system and resist such narrow, shortsighted, and ultimately harmful appeals.

Even more serious, however, for the future of America's scientific and technological capability is the possibility—I would argue that it is the likelihood—that the habit of treating scientific facilities as economic goods will lead to treating scientific projects as economic goods. There is a connection between a building and what goes on in that building.

As Dr. Sproull's testimony yesterday before the panel so ably explained, previous decisions on research facilities thoroughly recognized that connection in the decision process. It is inconceivable to me that facilities bargained for in the Congress on the basis of their connection to the condition of local economies will be allowed to lie fallow because the scientists in them are unable to compete successfully for project funds from NSF, NIH, DOE, or other Federal agencies.

It is essential that steps be taken now to avoid sliding into decisions about what science will be done and who will do it that are based on which district has the most effective representative, or which institution has cultivated members most effectively, rather than which science should be done and who is best able to do it.

The Congress has displayed remarkable forbearance with respect to such decision for nearly 40 years. Even faced with new pressures from constituents and others, it is within the ability of the institution to find new ways to cope. If there is within the Congress a disposition to adapt the real strengths of its old ways to the realities of national needs in the presence of scarcity, it must be based on the understanding that decisions about who will do what science where should be made only after the competitors for funds have been subjected to competent objective review by knowledgeable professionals.

I would want that rule to be close to inviolable with respect to decisions about particular projects or programs of research. In other cases—for example, those having to do with facilities—other criteria may be relevant to a final decision, but they should only be invoked with respect to those competitors judged to be qualified to do the work.

What that proposition would do is to make the accepted practice in dealing with national facilities the minimum standard in deciding on other facilities as well. The acceptance of such a self-denying ordinance by the Congress would be a helpful step toward policymaking.

I would also urge a second proposition with respect to facilities; namely, that the Congress will prevent the existence of a building from dictating decisions about project funding. If American science is to continue to flourish, we need, above all, to sustain mechanisms that support the best people in the best work, to the extent that competent professionals are able to make those judgments. To accept a lesser standard is to invite erosion of an enterprise on which so much depends.

I hope that this committee will seize the opportunity that it has created and lead the Congress to a new appreciation of its own strengths and limitations in treating issues involving science and technology. That would be a contribution worth our applause.

I thank you for your patience, Mr. Chairman.

[The prepared statement of Dr. Rosenzweig follows:]

## PREPARED STATEMENT OF DR. ROBERT M. ROSENZWEIG, PRESIDENT, ASSOCIATION OF AMERICAN UNIVERSITIES

Mr. Chairman and Members of the Committee:

I appreciate the opportunity to appear here today. I wish that I could discuss with you the full range of important and fascinating questions included in your study agenda under the heading of Science in the Political Process. I fear, however, that the limits on your time and my knowledge combine to make that impractical. Instead, I want to discuss with you an extremely important question of science policy, one that has taken on a new and special urgency in recent years. In its broadest and most useful form, that question is: What role should the Congress play in making decisions that affect the conduct of scientific and technological research?

You will note that this is not the related question that is so frequently asked, namely: what is the proper role of experts and specialists in making policy that has significant scientific content? That question has an ancient and honorable historical lineage and has been the object of a body of literature that has grown larger as the importance of science and technology has grown larger. The committee will necessarily confront the role of the expert in a variety of forms as it proceeds in this study. For now, though, I want to consider with you the role of the nonexpert--the Member of Congress--in making policies that will profoundly affect what science is done, where it will be done, and who will do it.

Let me start, then, by suggesting that questions about the role of Congress should really be understood to mean the roles of Congress. As science and technology have come to touch ever larger areas of modern life, they have evoked demands for public attention that. in a democracy, quickly become the concern of the public's elected representatives. The Congress can and often does play a number of different roles, each of them of potentially great import. For example, the Congress frequently must deal with regulatory issues of great significance to the conduct of science. To mention but a few that have arisen in recent years. there have been major debates over the regulation of recombinant DNA research, the use of human subjects in research, the use of animals in research, the propriety of fetal research, a variety of environmental issues, and the wisdom of controlling scientific communication in the interest of national security through the Export Control Act.

In addition to those regulatory activities, and sometimes overlapping them, the Congress has frequently played an active oversight and early-warning role. In recent years, there have been useful hearings on a variety of ethical issues arising from genetic engineering and the growing connections between universities and industry and, of course. the extensive study by this committee, of which this hearing is a part.

More conventionally, the Congress affects science by its regular authorizing and appropriating activities, and increasingly through its actions on tax policy, anti-trust policy, and patent and copyright policy.

I don't intend here an exhaustive catalog of the important ways in which the Congress, by what it does or does not do, affects the conduct of science. I think the point should be clear, though, that the health of science in the United States is intimately connected with the wisdom of a large number and wide range of decisions made in this legislative body.

It is important to understand, especially so for members of the academic and scientific communities, that the Congress is so deeply involved in science policy because many issues of science policy touch on important values and interests of the citizenry, and because they are frequently causes of conflict. Issues of that description often find their way into the political arena in a democratic society. That is not something to apologize for; it is something to take pride in, even though the processes of resolution are often messy and in some respects perhaps less satisfactory than some hypothetical ideal.

Having said that, it is also necessary to say that not all issues fitting that description are best dealt with through political instrumentalities, even though they could be. On occasion political leaders, like judges, refrain from involving themselves in issues for a variety of reasons, including the judgment that other means are clearly preferable, and to preempt those means would produce less desirable results and do a disservice to the public.

The task of the Congress with respect to its role in shaping science policy is to decide where to be active and where to exercise restraint; where its distinctive ways of reaching agreement will promote the public's interest in science and technology and where they will not. Since the Congress necessarily plays many roles, depending on the area of policy under consideration, this challenge is not likely to be met by a single position based on a single overarching principle. What is called for is the development of a set of expectations about Congressional behavior based on thoughtful consideration of the requirements of politics, the requirements of science and technology, and the nature of the decisions to be made.

At this point I must make an essential digression. Universities also have responsibilities and a role to play here. The questions must not be directed solely to the Congress.

If our research system is to continue to thrive and set the standards for research excellence, the task of universities is to gain a deeper appreciation of and respect for the role of the Congress in shaping science policy. No less than the members of the Congress, universities and their representatives need to exercise judgment and restraint as they present their individual and collective needs to their Congressional representatives.

Universities must be sensitive to the reactive nature of the Congress and must respect the necessary and legitimate responsibility of Members to respond to constituent needs. The vulnerabilities of Congressional processes should not be exploited for narrow institutional gain if the price of doing so is to weaken further the collective esteem for established and proven processes of allocating national resources for research. If the Congress needs to fashion a set of expectations about its behavior in the science policy arena, so, too, do individual institutions and their representatives. Self-restraint on both sides will help to prevent harsh economic realities and the resulting politics of scarcity from seriously weakening the foundations of our unique research enterprise.

Some in the Congress and elsewhere have wondered why the AAU and other voluntary organizations do not impose sanctions on members who are driven by scarcity and opportunism to make direct appeals to the Congress. From personal experience I can assure you that there are real and important limits on the ability of voluntary associations to control the behavior of their individual members. They simply lack the means to do so without engaging in self-destructive behavior. That is why the mutual understanding and self-restraint of the Congress and the university community are essential.

What may be surprising to the committee as it considers this matter is how little attention has been given to the role of

Congress in science policy. In sharp contrast to the rich literature examining the role of expert knowledge, I would submit that this committee will have great difficulty in finding guidance from systematic, empirical, or normative analysis of Congress's role in this important domain.

In the last year, we have learned just how unfortunate that inattention has been. As this committee well knows, we have been engaged in a noisy controversy about one aspect of the Congressional role, that having to do with the allocation of funds for scientific facilities. The controversy involves a number of instances in which the Congress has made appropriations for the construction of research facilities at particular universities without either a competitive application process or a professional review prior to approval. The practice appears to have grown in the last two years. Some, including those who have benefited from it or hope to, say that it is a perfectly legitimate, indeed time-honored, way of doing business. They point to a number of instances in the past in which particular decisions can be traced to the influence of a powerful Congressional patron, and they profess to wonder why there is such a fuss simply because what was once done behind the scenes has now become more visible. Others, of whom I am one, say that this reading of the record in fact distorts it, that a fairer reading of the record will show truly remarkable Congressional self-restraint in dealing with decisions on scientific facilities, and that actions of the last two or three years mark a change in behavior which, unchecked, will be inimical to the public's interest in supporting high-quality science.

It has been, until now, an unsatisfactory debate. History has provided no authoritative guide to past practices because no reliable history exists. More fundamentally, the debate has gone on as if it had no connection to the general question of what role the Congress can and must play in forming science policy if the system is to operate properly and, conversely, what actions the Congress should refrain from taking for the same reason. Consequently, we have seen a hopeless muddle over the difference between the right to take a particular action and the wisdom of doing so. Self-interest has flown under the banner of the First Amendment right of citizens to petition the Congress and the corresponding power of the Congress to respond as it chooses; cynicism about universities and science has grown in the Congress and in the press. and cynicism about the Congress has grown in the press, in the universities, and among scientists. I don't think "unsatisfactory" is too strong a word.

And unsatisfactory it will remain unless we can erect a strong and defensible framework for building a thoughtful and useful role for the Congress in science policy, a role that is consistent with the history and traditions of our political system, that meets the legitimate political needs of Members of Congress and defends them against illegitimate demands, and that helps sustain the highest quality scientific and technological work of which our people are capable. This committee has an opportunity to make an important contribution toward that end. I would like to spend the few minutes remaining to me here by making some suggestions about the contours of that role.

Some propositions seem to me relatively easy and non-controversial. There is little remaining argument over the proposition that the federal government must be centrally involved in the support of fundamental research and training for research in science and technology over a wide range of disciplines. That being so, committees of Congress will be regularly involved in science policy through the usual activities of authorization and appropriation.

Nor is there dispute any longer over the legitimacy of Congressional concern for regulatory issues in science. We are long past the time when it could be argued that the imperatives of science and technology sweep aside all other social values and community interests. Environmental and safety concerns, as well as ethical considerations in the uses of new knowledge, are widely seen as legitimate issues for deliberation in the Congress.

If legitimacy of a Congressional role is not at issue in these areas, the wisdom of any particular action, or of the need to take action, very likely will be in every instance. I take that not as a denial of Congressional responsibility, but as reassuring evidence that the Congress will not be left to discharge its responsibility unchecked by public scrutiny and debate.

I suppose that one could say that the controversy of the last two years also involves an area in which the legitimacy of Congressional action is not at issue. Certainly it is true that in other policy domains in which money is distributed, active involvement by Members in who gets how much is the norm. That involvement is not limited to an interest in setting distribution formulas, but frequently reaches directly to decisions about the location of individual projects.

In general, Members of Congress have not been involved in decisions about scientific projects to nearly that degree. Decisions about which scientific projects should be funded have been made almost entirely without direct Congressional involvement, and almost always only after competent professional review of the merits of the work to be done. The record with respect to facilities is more mixed, but it is mixed in a reasonably orderly way that suggests the existence of at least tacit understandings about what is appropriate Congressional action and what is not.

The Committee on Science, Engineering and Public Policy of the National Academy of Sciences, in its recent report, "The Outlook for Science and Technology 1985," illuminates that order by classifying four kinds of facilities projects:

1. National facilities, intended to serve a national, often international, research community--for example, the Fermi National Accelerator Laboratory in Illinois.

2. University-based research facilities; a new or renovated chemistry or engineering building is an example.

3. Regional research facilities usually based at a university; the report cites the Triangle Universities Nuclear laboratory in Durham, North Carolina, as one example.

4. Technology centers; these are usually located at or affiliated with universities that are tied to local or regional economies--for example, the Basic Industry Research Institute at Northwestern University.

Items one and two on that list can be seen as end points on a continuum. Items three and four lie somewhere in between. At one end of the continuum lie the large national facilities, which because of their very size have significant economic benefits for the communities in which they are located. The decisions on where to put those facilities have usually followed heated political struggles, and no doubt they always will. Almost without exception, though, the battle has been waged among competitors that have been chosen from a larger group, using criteria of scientific quality as judged by those who are qualified to make such a judgment.

At the other end of the continuum lie the many decisions about campus facilities for the conduct of programs of research. These were the meat and potatoes of the government's facilities support programs of the 1960s, and the disappearance of those programs in the late '60s is one important explanation of the pressures that Members of Congress are now feeling. There was and is no sense in which these could be called national facilities, and in most cases they were not really even regional facilities. Instead, they were essential elements in the capacity of individual institutions to do good research. It was expected that the research done in them would very largely be competitively funded research projects for which faculty would apply and whose quality would be judged by other scientists. Indeed, one criterion of success in winning facilities funds was the judgment that the research to be housed in the building was likely to be of high enough quality to win such awards.

What marks the middle two categories is their connection to local or regional economic development. The criteria for locating facilities that are thought to be important to plans for economic development are bound to give weight to considerations other than scientific quality. Economic goods are political prizes and will be fought for as such. However, that still leaves unanswered the question: What are the rules of battle?

That has become a critically important question, because what we have witnessed in the last two years is a blurring of the reasonably clear categories that the Academy report described.

Part of that blurring is, it should be said, tactical in nature. The Columbia University chemistry building, for example, which was authorized by floor amendment to the DOE Authorization two years ago, was described as the National Center for Chemical Research because, I would guess, a stronger case could be made for Congressional action on a national facility than on a merely very distinguished chemistry research program of a single university. But in larger part, the blurring represents a recent and quite widespread belief that science and technology must be the centerpieces of modern economic development. As more states and localities come to believe in that relationship, they will turn to their universities to be leaders of their development programs and to their representatives in Congress to provide the wherewithal for university participation, just as they have long done for flood control, rural electrification, hydroelectric power, and other large capital projects essential to local economic well-being.

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Even more serious, however, for the future of America's scientific and technological capability is the possibility--I would argue that it is the likelihood--that the habit of treating scientific **facilities** as economic goods will lead to treating scientific **projects** as economic goods. There is a connection between a building and what goes on in that building. As Dr.

Robert Sproull's testimony yesterday before the Panel so ably explained, previous decisions on research facilities thoroughly recognized that connection in the decision process. It is inconceivable to me that facilities bargained for in the Congress on the basis of their connection to the condition of local economies will be allowed to lie fallow because the scientists in them are unable to compete successfully for project funds from NSF, NIH, DOE, or other federal agencies. It is essential that steps be taken now to avoid sliding into decisions about what science will be done and who will do it that are based on which district has the most effective Representative, or which institution has cultivated Members most effectively, rather than which science should be done and who is best able to do it.

The Congress has displayed remarkable forebearance with respect to such decisions for nearly forty years. Even faced with new pressures from constituents and others, it is within the ability of the institution to find new ways to cope. If there is within the Congress a disposition to adapt the real strengths of its old ways to the realities of national needs in the presence of scarcity, it must be based on the understanding that decisions about who will do what science where should be made only after the competitors for funds have been subjected to competent objective review by knowledgeable professionals. I would want that rule to be close to inviolable with respect to decisions about particular projects or programs of research. In other cases--for example, those having to do with facilities--other criteria may be relevant to a final decision, but they should only be invoked with respect to those competitors judged to be qualified to do the work.

What that proposition would do is to make the accepted practice in dealing with national facilities the minimum standard in deciding on other facilities as well. The acceptance of such a self-denying ordinance by the Congress would be a helpful step toward orderly policy making.

I would also urge a second proposition with respect to facilities, namely: that the Congress will prevent the existence of a building from dictating decisions about project funding. If American science is to continue to flourish, we need, above all, to sustain mechanisms that support the best people in the best work, to the extent that competent professionals are able to make those judgments. To accept a lesser standard is to invite erosion of an enterprise on which so much depends.

I very much hope that this Committee will seize the opportunity that it has created and lead the Congress to a new appreciation of its own strengths and limitations in treating issues involving science and technology. That would be a contribution worth our applause.

## DISCUSSION

Mr. FUQUA. Dr. Rosenzweig, we have a vote and we will recess for a few moments while we respond to the vote; then we will resume with the questions.

Dr. ROSENZWEIG. Thank you.

[Recess.]

Mr. FUQUA. The task force will resume.

Dr. Rosenzweig, your statement throughout was replete with the wish that Congress not get involved, but there are political forces in town that also operate. It can be the Executive Office of the President; it can be agencies that have their own political reasons for doing things, that may not object to things happening that way.

But do you think the peer review system as we see it today, has it helped to broaden the basic research throughout the country? When you look at some of the figures of those that have been the most successful in getting Federal programs, they continue to be the most successful. And yet you find in some cases schools that are emerging or have put great financial efforts by State or private funds into trying to upgrade their schools; yet they are not up into that top 50 category. Has it served to broaden that?

In the charter of the National Science Foundation, it states: "In exercising the authority and discharging the functions referred to in the foregoing subsections, it shall be the objective of the Foundation to strengthen research and education in the sciences, including independent research by individuals, throughout the United States, and to avoid undue concentration of such research and education." Has that been carried out?

Dr. ROSENZWEIG. I think that the system we have developed and operated in those agencies has produced the best balance of high-quality research and reasonably wide distribution of research funds that one can get out of a system that is developed and operated by fallible human beings.

Let me approach it in a couple of different ways. It is frequently said that the rich get richer and it is impossible for those whose noses are pressing against the candy store window to get inside the candy store.

Well, in fact that is not the truth, or at least it is an oversimplification. Within the last 20 years, let's say—not an unreasonable period of time—let me cite the following institutions that have gone from being good institutions to being quite distinguished institutions and have had that recognized in Federal funding.

The leading example probably is Stanford, which was a reasonable regional institution maybe 25 years ago and is now a first-class institution. But in addition to that, I would cite, for example, UCLA, the University of Arizona, the University of Florida, the University of Utah.

All of these are institutions that have made major strides over the last two decades, helped by—not hindered by, but helped by—the fact that when they got through their own efforts and resources, State efforts, local fundraising efforts, when they got to the point at which their faculty were competitive for grants, they got grants, and the reason they got them is because the system is essentially a fair one. On the whole, to a remarkable degree, it rewards quality. It seems to me we cannot ask for anything more than that from a system.

I would also say, if you want to know why more institutions don't make it—it may seem frivolous, but let me put it this way. Sports writers wonder why the Boston Celtics and the Los Angeles Lakers, year after year after year, make it to the finals of the NBA championships, even though the system in the NBA is rigged against that sort of thing happening. The last are first in the draft, and the first are last. They put on a cap on the total salaries that any team can pay. And it doesn't matter. Year after year, the same teams turn out on top. Why is that?

Well, it has something to do with good management. It has something to do with aggressiveness, with the ability to judge talent, with local support of a team. There are a lot of factors involved that cannot be changed simply by jiggering the system to produce artificial advantages.

Mr. **FUQUA**. When certain aspects of activities such as science are exempted from political determination, what happens to the political accountability for individual legislators or Governors or Senators?

Dr. **ROSENZWEIG**. That is an excellent question, and I hope I am not misunderstood as believing or arguing that anything that the Government touches is going to be free from political influence. I mean, that is like arguing that you can stand up and float off into space, contrary to the laws of gravity.

There will be politics. The question is how the role of politics ought to be shaped and defined in this area as opposed to other areas, and I think that there are some distinctive characteristics of science and the public's interest in science that require a kind of self-restraint by Members of Congress that has been amply rewarded in the past. The Nation has prospered in part as a consequence of those acts of self-restraint, and that will continue to be rewarded in the future.

I don't think we are going to banish politics. I would be opposed to it. I mean, I like politics. I think it is fun. It is one of the reasons I am here. But I think it operates in different ways in different areas of public policy.

Mr. **FUQUA**. How about in cases like at NIH, where there has been great public pressure to create different divisions within that addressing specific diseases—arthritis, cancer, heart, and stroke? Would you classify that as too much Federal involvement? That didn't say where the money was going specifically, but it addressed certain topical areas.

For instance, another one that was mentioned this morning was the Apollo Program. That was a political decision. It was backed up by engineering and scientific information that said it was achievable within a certain period of time, but that was a political decision, for many, many reasons. Are those improper decisions on the part of the political process, or are those within the realm?

Dr. **ROSENZWEIG**. No, I don't think they are. Any one of them may have been a wrong decision, but I don't think——

Mr. **FUQUA**. Oh, the medical profession, for instance, opposed in the *NIH* case.

Dr. **Rosenzweig**. Yes, and still would. At least some parts of it would.

Mr. **Fuqua**. Yes.

Dr. **Rosenzweig**. And, in general, you would find us—that is, universities and medical societies—opposing proposals for new institutes, and there are reasons why it is undesirable to do that. Some would argue—in fact, it may even be the dominant view—that creating such institutes is an inappropriate exercise of congressional power.

I don't happen to believe that. I think it is a perfectly appropriate exercise of congressional power, although it may be misguided in any particular case.

Mr. **Fuqua**. But the Congress, through the political process, has the responsibility to extract the funds from the taxpayers to support these programs, and maybe by doing that, then we have broadened the constituency support for certain programs.

Dr. **Rosenzweig**. I think in fact that has happened; you are absolutely right.

Mr. **Fuqua**. Now, whether it would not have happened, I don't know.

Dr. **Rosenzweig**. You are absolutely right. That has in fact happened, yes.

Mr. **Fuqua**. Well, the same thing happens when we look at some of the geographical distribution of some of the funds within the granting agencies, where hardly a year goes by—Mr. Walgren, I think, will attest to that—that we don't have members of our committee and other Members on the floor complaining that they feel that their region or their State has not been dealt with fairly in the distribution of the funds; that it is not going to not incompetent places.

For instance, we had one witness a few weeks ago who was talking about there are some big projects, like if you get into nuclear physics and so forth, that require big equipment and very expensive projects, but that chemistry was one that probably some of the better chemists in the country came from some of the smaller colleges; that chemistry was a very strong subject among the less large schools, if you look at them in terms of Federal dollars. Would it be wise to try to have programs to help those schools advance, say, their chemistry programs?

Dr. **Rosenzweig**. I don't see any reason why not, so long as the programs are——

Mr. **Fuqua**. As long as we are getting good science.

Dr. **Rosenzweig**. Yes, that is right, and the programs are run competitively.

Mr. **Fuqua**. But would peer review be adaptable to that?

Dr. **Rosenzweig**. Yes, absolutely, absolutely.

One point on which President Silber and I agreed was when he quoted John Locke as saying that no man should be a judge in his own case. I take that to mean that when a university president or a college president comes and tells you how good his research program is, you ought to give him a respectful hearing and then go ask somebody else, and that is what peer review or professional review is supposed to do. It is supposed to give you, or whoever makes the decision, somebody else, somebody who is not a party to the case, to rely on in making those decisions, or at least narrowing the field from which the decisions will be made.

Mr. **Fuqua**. Well, do you think there is a separation between the basic research grant, where you are doing pure science, and facilities? We have not had a facilities program, as you mentioned, since the sixties. Now there is great pressure to do something about facilities. Many of them are over 25 years old; some of them date back prior to World War II.

Is there a difference between those, or should the facilities be subjected to the same process that we do for pure science?

Dr. **Rosenzweig**. I think it is a difference in degree rather than a difference in kind, Mr. Chairman. I think that in both cases, the primary consideration is the quality of the work that is to be done.

As I indicated in my testimony, in the case of the research project, that ought to be, in my view, virtually the sole criterion. In the case of facilities, other criteria are admissible but, in my view, after the question of quality is decided, and the field is narrowed to those that meet the minimum standard that is judged to be acceptable. Then it seems to me that other criteria are appropriate.

It does make a difference where economic advantages are distributed in this country. It would be foolish to deny that, or to try to erect a system that kept that consideration out. The question is at what point you admit it and how much weight you give it.

My view is that when you are distributing scientific facilities, the primary consideration ought to be the quality of science. That, in my view, is the primary reason why the public is willing to pay for all of this expensive activity, because it gets results out of high quality science. It is going to get less results out of lesser quality science. So the quality ought to be the first consideration. Given the existence of quality, then it seems to me that other criteria are admissible into the decision process.

Mr. **Fuqua**. Well, it varies from jurisdiction to jurisdiction, but in the Federal court system, we have a very complicated jury box list that they choose people to serve on juries; they pull their names and they are summoned for jury duty. It tries to bring in all of the citizens.

Is that what the peer review does? I am talking about scientists. Do they have a broad base, or should that be broadened in the peer review process to judge the various proposals on their merits?

Dr. **Rosenzweig**. I am not sure I understand. Do you mean should other criteria be——

Mr. **Fuqua**. No, what I am saying is, should there be a broader representation that, say, the Science Foundation or NIH could call upon to evaluate?

Dr. **Rosenzweig**. I see. My impression is that they do in fact make an effort to draw their peer review panels quite broadly from the scientific community, and they are not limited to the major universities. But that is a question of fact. I mean, they can give you the data on that, and you ought to get it from them and make the determination yourself as to whether you think that is an acceptably wide distribution of participants. I think it is fairly wide, that they are reasonably attentive to that.

There is some material in the readings that the committee was given by the Congressional Research Service on the evaluation of peer review. Both the research in there that is supportive of peer review and the research that is somewhat critical of it bears absolutely no relationship to the description that we have heard this morning elsewhere.

It is not a perfect system, but it is not a conspiracy, either. It is not a way for the rich and the powerful to keep the poor and the helpless out of the mansion. It just hasn't been that, and I don't think it is that now.

Mr. **Fuqua**. Mr. Walgren?

Mr. **Walgren**. Thank you, Mr. Chairman.

It seems maybe at some point in this study we ought to try to find out a little bit more about how the peer review people are chosen, and I don't know anything about that. Somehow I have images of choosing lotteries on the television, where a ping pong ball pops up with some air pressure or something like that, but even that was fixed in Pennsylvania one time when we had that. [Laughter.]

Mr. **Fuqua**. I might point out, we do have a hearing on that in the spring of next year, on peer review.

Mr. **Walgren**. And maybe some conclusions could be drawn from that. I guess I instinctively, from the outside, want to take a skeptical approach and sort of wonder who chooses those people. Concentrated choosing can make something go awry as well as—even though, in theory, the thing should function properly.

And then, as I understand it, in some instances we really do lock ourselves into the peer review system at NIH. I gather that is the case, that they have an outside peer review system which is binding on the agency.

Dr. **Rosenzweig**. No, it can be changed above the level of the study section.

Mr. **Walgren**. Is that right?

Dr. **Rosenzweig**. Yes. Changes can be made.

Mr. **Walgren**. But there is apparently some contrast between NIH and NSF, where at NSF it is really only a recommendation, and there are abilities to direct the distribution differently.

Dr. **Rosenzweig**. I think that is right, and I should make it clear that peer review doesn't describe a single thing. There are different ways of doing peer review in different agencies, and I personally tried not to use the term "peer review" too broadly but to use the term "professional review," because in some agencies, like the Department of Defense and, I guess, the Department of Energy, outside peer review panels are not the common mode of making judgments, but other kinds of professional scientific review are used that are quite acceptable.

Mr. **Walgren**. How do you define what kind of professional review is acceptable? I gather—for example, you suggest that in placing the national facilities, there is obviously a role for politics, and in fact Stanford is pursuing a very political effort to secure the supercollider or whatever it is.

Dr. **Rosenzweig**. It is part of a consortium, as I understand it.

Mr. **Walgren**. At least I understand it is part of a consortium where the State government has put up a substantial amount of money in order to lobby that decision.

Dr. **Rosenzweig**. Yes.

Mr. **Walgren**. And yet you would require that that possibility be one of several who have been professionally reviewed in some way?

Dr. **Rosenzweig**. That is right.

Mr. **Walgren**. And I guess the question is, What kind of professional review is adequate for that? Is the President's Science Adviser adequate, and would that be professional review, or shouldn't we set up some kind of structure that builds in this input in a clearly acceptable way and one that many people who now perhaps don't have confidence in the system would have more confidence in the system?

Dr. **Rosenzweig**. That is an interesting case. I would have to be a lot more foolish than I am to think that that decision is going to be made without substantial congressional jockeying over who gets the final award, assuming it goes that far.

But I am also certain that before that happens, there will be a very detailed and exhaustive evaluation of the maybe 50 proposals that will come in from various parts of the country, submitted to be the host for that facility, and that that number will be winnowed down to perhaps half a dozen, and that is what the fight will be over.

Mr. **Walgren**. But who will winnow? I guess that is the question.

Dr. **Rosenzweig**. Oh, there will be panels of distinguished high energy physicists and theoretical physicists and mathematicians, whoever else is involved in those.

Mr. **Walgren**. And they will be appointed by——

Dr. **Rosenzweig**. The Department of Energy.

Mr. **Walgren**. By the Department of Energy?

Dr. **Rosenzweig**. Yes.

Mr. **Walgren**. And that is all right, from your point of view?

Dr. **Rosenzweig**. Oh, yes. I have no problem with that.

Mr. **Walgren**. There is no structure to that that you feel could be recommended?

Dr. **Rosenzweig**. No particular structure. They have done it in the past, and I think it has worked reasonably well in the past. There is a track record for that sort of decision.

Mr. **Walgren**. Well, of course, the problem is, many would allege that it hasn't worked that well in the past, I guess, and they are suspicious. For example, in the supercomputer testimony that was raised earlier, the primary driver for the program got a supercomputer, and that—it is clearly an expert in the area, but it was a supercomputer that was not even one of the—was sort of a special supercomputer, one with a special mission as opposed to the missions that the rest of the centers were competing to perform.

It would seem to me that some recommended structure is necessary to prevent people from feeling that that is a suspect process.

Dr. **Rosenzweig**. I guess I agree. I was not involved in it, and I am not really familiar with the details of the process of that competition. If it was flawed—it is always hard, Mr. Walgren, to persuade the losers that they lost for some reason other than that they were judged not to be the best of the competitors.

Mr. **Walgren**. But that is what process is for.

Dr. **Rosenzweig**. Yes, I agree. The question I am raising is whether there is any process that will assure everybody that did not win——

**Mr. Walgren.** And I doubt that, and I would give you that, but I don't know that we can just say, "Well, we want professional review," and then just let it to whoever designs the professional review at the time and say, "We are satisfied with that."

It would seem to me that in advance of that, we ought to be able to agree on some kind of breadth of professional input that we could ask everyone to live with in advance of the decisionmaking process. But I think your point that professional review is part of it certainly has a lot to recommend it.

Let me ask you, in your testimony you indicated, talking about the programs in the sixties for facilities, and there is a sentence in there that indicates how essential it is that the idea was that these facilities, even though they fell in the middle of your continuum—which supposedly would be away from direct political influence—nonetheless, these facilities were viewed as essential elements in the capacity of the individual institutions to do good research. And so if you didn't get that essential element, then you weren't going to be able to do good research. It literally left you without capacity to do good research.

I wanted to ask, historically—you go on to say that these facilities were distributed after a judgment that the research to be housed in the building was likely to be of high enough quality to then win the future competitive awards that seemed to follow on.

How was that judgment made? Who made that judgment that they were likely, that the future research was likely to be of high enough quality?

**Dr. Rosenzweig.** Site visitors would come out from NSF or NIH or NASA, whatever the supporting agency was, and talk—they would have, obviously, voluminous written materials describing the research programs and productivity of the faculty who would use the buildings, and they would talk to the faculty and get the best sense they could of what was likely to go on in the building after it was built.

Buildings do not make research. Good research can be done without buildings. Not for long, and it is increasingly difficult to do as science gets larger and more complicated. But I think it is incorrect to say that an institution that doesn't have first class, most modern facilities cannot bring itself to the position at which its faculty can be competitive for research grants.

It is not simple. It is not a neat distinction. But I think that if we were to arrive at a system, for example, in which every one of, let's say, 250 universities were given enough money to build, each, five new science buildings, I think you would find, at the end of 5 or 10 years, that maybe 100 of those would have turned out to be productive, to use those facilities productively, and that the rest will be chugging along with not much going on.

I think you have to make the judgment first about whether the work that is going on and is likely to be carried on, given the existence of a new facility, is going to be worth doing before you put that money there.

**Mr. Walgren.** Yes. You know, I think that there ought to be some way where we could have that element in things, but it seems to me the judgment of whether the future work is likely to be of high enough quality to win some future competition, that is a very doable yardstick, I would think, for most institutions; that if you went and made a site visit, you are not really in a competition at that point. You are not being matched off against the merits of your research proposal at that point at all, but you are just kind of establishing your credentials, I gather, but not competitive.

It would seem to me that that is a level of professional selectivity that could be worked into this process very easily and to your satisfaction, the AAU's satisfaction, and to the satisfaction of almost all the other institutions.

I guess what I would wish is that, somehow or other, we could work toward how to structure a process that looks at that level. I am uncomfortable to say that: "Look, we as scientists know exactly where everything should go," and it is done in a peer process, and even a competitive process, when a lot of breadth of our society is left out in a competition, and when I think about what built the base of our present research structure, it was not that kind of intense competition, but rather a satisfaction that the research that would go on was likely to be of high enough quality to compete in such an area.

And if that is the judgment we make about where facilities go, it would seem to me that we could figure out a way for professional views to be brought to bear on that to everyone's satisfaction.

But I think it is important to underline the danger of the present debate, as you did in your testimony, because there are some people who are apprehensive now, and they are not limited to President Silber, but others that have expressed their apprehension to me about how they might be received at various levels that are important to them, and that is really antithetical to our scientific enterprise if what we are involved in is the present pursuit of scientific merit.

Dr. **Rosenzweig**. I am truly distressed to hear that and would be surprised if there were any substance to it.

Mr. **Walgren**. I think that is important, to know that you view that kind of apprehension with surprise, and in fact, you feel that any apprehension would be groundless, literally. I think that is a very important commitment for someone in your position with the AAU and the like to be making publicly.

Dr. **Rosenzweig**. You have it as categorically as I am capable of making it.

Mr. **Walgren**. Well, I appreciate that rumination with you. I wish that in the process of this study we might pursue whether there is some process that brings what you call professional judgment to bear, that gets us away from the problem of having to be in such an intense competition that large hunks of society are left out.

Dr. **Rosenzweig**. Thank you.

Mr. **Walgren**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Dr. Rosenzweig, thank you very much for being here this morning.

Dr. **Rosenzweig**. Thank you, Mr. Chairman.

[Answers to questions asked of Dr. Rosenzweig follows:]

## QUESTIONS AND ANSWERS FOR THE RECORD

Dr. Robert M. Rosenzweig

1. **Have there been any other periods or cases in our history when the question of the best balance between scientific expertise and political judgment has been the subject of debate?**

Recent history is full of examples of disputes between expertise, including scientific expertise, and political judgment. Two examples come to mind that are interesting because they are so very different. One that has been in the news again, recently, was the effort of some of the scientists who worked on the Manhattan Project to persuade President Truman not to use the A-Bomb. That argument has in all of its essentials continued to the present day and can be seen in debates on arms control and SDI, among other issues. It is an argument that takes place on political territory, but that has scientific content. The experts claim attention not because the subject matter is so very technical, but because their knowledge gives them an understanding of consequences beyond that which is readily available to others. It is a very useful debate; without it, American strategic policy would be much worse than it is.

An example of a different kind arose in the immediate aftermath of the invention of Recombinant DNA technology. It was followed by public--or at least media--hysteria about the prospect of new and terrible organisms being created in the laboratory and loosed on the public by accident. Congress became involved in that debate, with the weight of Congressional opinion responding initially to the public outcry, in opposition to the preponderance of scientific opinion. No regulatory legislation emerged, though it has always seemed to me likely that the very fact of the debate served to reassure the public and relieve pressure for unwise action.

2. **Have there, to your knowledge, been comparable cases in other fields, such as law, medicine, defense policy, social security, and welfare policy, where the balance between professional judgment and political judgment has been at issue?**

Some professional judgments and some political judgments are at odds in almost every field of public policy. That has long been the case, and the conflicts will only increase as public issues increasingly come in specialized and often technical form. It should be remembered, too, that professional opinion is often a political weapon, and a valuable one. Political leaders vie for the most prestigious figures available to endorse their positions, and professionals frequently seek out politicians who will carry their views into the political arena. In such fields as defense policy and economic policy, that phenomenon is so common as to be totally unremarkable. In medicine, ethical issues often pit physicians against laymen, and the field of law is in constant tension between the views of professionals and those of nonlawyers. The conflict is by no means unique to science. nor, as I have indicated, is it necessarily or entirely unhealthy.

3. One of the traditional rules of the political world, arising from the concept of representational government, is that legislators have a duty to look out for the interests of their constituents. This is a particularly strong tradition in our American system where we have no members elected "at large" or on the basis of total party votes, but all represent specific geographic areas and the people living within that area. Should we expect legislators in such a system to exempt certain parts of the Federal Government's activities, be it science, or defense, or the arts, or any other field, from such political interest or influence?

There are many ways to express the dilemma of the elected representative in balancing the interests of his constituency--and therefore his own immediate political prospects--against the interests of the nation. All elected legislators face that dilemma, and I would guess that very few would want to argue that constituency interest overrides national interest whenever the two are in conflict. Nor would the system permit pure adherence to the national interest--assuming that can be known--over constituency interests when they clash. If either pure case were possible, there would be no dilemma, but only a simple decision rule for all to follow.

In that setting, the issue is not whether this or that area of policy ought to be exempt from considerations of constituency interest--none is or should be--but how the balance should be struck in each area of policy. In very large measure, the posture of the Congress with respect to science funding for the last forty years has been to forego seeking constituency benefits from decisions about basic science funding. In my testimony I noted exceptions and suggested some guidelines for the future, but I believe that a decision to change the balance will do damage to the nation's interest in quality science.

4. If certain aspects of federal activity, such as science, are exempted from political determination, what happens to the concepts of political accountability for the individual legislator?

Again, I am not arguing for exemption, but for self-restraint. If that practice has produced breaches of political accountability, I am not aware of them.

5. Those who have expressed a concern about efforts to obtain research facilities by a resort to the political process have noted that some projects have been initiated not only without scientific review, but also in the case of floor

**amendments, without review in the Congressional system; that is, through subcommittee and committee hearing and debate. Would legislative provisions for such facilities be more acceptable if they were the result of such a more extensive Congressional review?**

There are sound arguments within any institution for following regular processes and for resisting their circumvention. In general. in my view, Congress works better when its committees are enabled to do their proper jobs. I have no doubt that such is the case with respect to science policy also.

But institutions also have distinctive functions and characteristic abilities that grow in response to their functions. Nothing that I have observed in the Congress leads me to believe that. as presently constituted, it has the ability to render competent and credible judgments about the scientific merits of competing proposals. Indeed. to the extent that constituency interests are asserted as the basis for a Congressional role in decision-making, and therefore come to dominate the process, even the possibility of dispassionate judgment is lost. One set of interests is simply asserted against another and acceptable accommodations are made among them. That works perfectly well in some policy areas; it would not in this one.

6. **How would Members of Congress not serving on the relevant Committees have equal opportunities to influence the provisions of a bill if floor amendments of the kind we were discussing would be out of order?**

It is almost never the case that all Members have opportunities equal to committee members to influence provisions of a bill. In order to make the committee system work, Members frequently yield to the superior knowledge and experience of committee members and follow their lead. More to the point. though, since my own view is that it is unwise to target funds for individual facilities and projects without prior professional review, even when it is done by committee action, the issue raised in the question does not present a problem for me.

7. **Based on your observations and knowledge, do state legislatures generally reject the practice of seeking university facilities through the political process at that level, or are such legislative provisions concerning science projects and facilities widely used at the state level?**

Let me speak to this question from my experience in California, the only state I know well. In the 1950s and '60s, the Legislature was extremely active in decisions about where to place new colleges. All such decisions were the product of hard political in-fighting. In recent years, it has been my impression that decisions about facilities at the University of California have been left to the Regents and the University

Administration. Indeed, legislative targeting would probably be resisted as an unconstitutional intrusion into the independence of the University. Except for the constitutional argument, I think that approximately the same thing is true for the California State University system.

8. In the event we continue to see the practice of lobbying for research facilities or other attempts to make judgments about science within the political process at about the level of the past 3 to 4 years, what, if any, will the long-term effects be, in your opinion? Is there a threshold level where this practice has serious adverse effects, in your view?

The point I would want to emphasize is that I do not think it is possible to hold the practice of targeting at any fixed level, nor do I think it is possible to limit the practice to facilities funding. With respect to the former, the dynamic I see operating is that every example of success through lobbying the Congress increases the pressure on both university officials and Congressmen not to be left out. Everybody has some political influence, and if that is the new game, nobody will want to be the last to play it. The pressure is irresistable.

The point to be made about the latter is that the distinction between a facility and the work to be done in a facility is often thin, and the same pressures that lead to lobbying for the facility will begin to operate with respect to project support. I have not yet heard a single persuasive argument as to why the politics of the two should turn out to be different.

9. It is being argued that the Congress should provide general goals and guidelines for science programs, including science facility programs, and let the agencies of the Executive Branch do the actual selection of the institutions and individuals to receive support. Yet in other areas where the Congress provides support, most notably in the defense procurement and the defense construction areas where a high degree of expertise presumably also is needed, the legislative provisions are highly detailed ("$18 million for M-1 heavy trucks," "735,000 for a refrigeration building at Tyndal Air Force Base"). Is there any evidence that one system is notably better or worse, or are the different approaches mostly a matter of tradition and practice?

I am hardly an expert on defense procurement. My knowledge is limited to what any attentive reader can glean from newspapers and magazines. My reading of that material leaves me in no doubt that for science funding to move in the direction of defense procurement would be the end of first-class science in the United States. There may be no better alternative to the existing system in the defense area--I am not a good judge of that--but there is a better alternative for science funding, and that is a system that puts primary emphasis on screening for high-quality performance. We know how well that system has worked. We can also see the consequences for cost and quality of a military procurement system in which other considerations are primary. Why on earth would we want to trade one for the other?

10. **Apart from the question of the respective roles of the members of the political world and of the scientific community, what means do we as legislators and you as members of the scientific community have to "enforce" or at least encourage compliance with whatever policies or guidelines that we may arrive at in this area?**

Personally, I do not like the "enforcement" model. The system until now has not been leak-proof. Rather, it has been sustained by a very general belief that it is essentially fair with respect to assessments of quality. So long as there was enough money available to assure a reasonable distribution of funds, neither institutions nor Members of Congress had much incentive to evade the system. Clearly, the absence of federal facilities funding for nearly fifteen years has strained the fabric of agreement. If something like H.R. 2823 were to pass and be adequately funded, I think the system would right itself again. That is an optimistic judgment; it is also possible that the political genie is now out of the bottle and can never be put back. To concede that, however, would be to give up too much too soon.

11. **Would it make any sense to establish a cost limit for science projects and facilities below which legislative specifications should not be made, but above which statutory specifications would be appropriate?**

This has been the case de facto for some time. Large national facilities are often subject to separate authorization. The important question is what precedes authorization and appropriation. So long as legislative action is preceded by serious professional review, I would be prepared to take my chances on what would follow.

12. **Should priorities within a science program be established strictly on the basis of intrinsic merit, or should added weight be given to areas of research that promise rapid advances--even at the expense of equally meritorious work in other areas? In other words, should peer review operate only to evaluate merit or should it also help establish priorities?**

The informed opinion of specialists is useful both in setting priorities and in allocating funds within established priorities. The former is, however, an intrinsically political act for which policy makers can use advice. For example, the decision to put a man on the moon was nothing if it was not a political decision in the broadest sense of that term. It would have been nonsense, however, if the weight of informed opinion had been that the task was impossible scientifically or technologically. That is precisely the argument now in progress with respect to SDI. These are not decisions for scientists to make, but politicians make them at their peril if they ignore the counsel of experts. Having made the political decision to follow a course of action, prudence dictates that the weight of expert opinion about how to do it and who is most likely to get it done should increase. There is a real-world reality to specialization and division of labor. Institutions, including the Congress, do some things better than they do others, even though they may have the power to do those things that they are less well equipped to do. I am arguing for recognition of that fact of life and for conduct that reflects it.

13. **Can the peer review process be extended to yield a broad consensus not only about research priorities within disciplines, but also about priorities for both moderately expensive research facilities and very expensive "big science" facilities?**

The term "peer review," if it must be used at all, should really be limited to situations in which a group of professionals is asked to evaluate the work, either completed or proposed, of an individual in the same field. There are many other purposes for which groups of specialists may be convened which are not, by that definition, peer review. One such purpose is to solicit advice about research priorities generally or about the need for a new accelerator or about some other question of policy. In such cases, professional advice can be an aid to reaching agreement, but it is not a substitute for other kinds of judgment. The formal application of the peer review system would come at a later stage, as, for example, when decisions need to be made between competitors for a project or facility.

The reason for being so fastidious about the distinction is that the peer review system, while well adapted to the task it is able to perform, can only be hurt if it is stretched to do other tasks for which it is less well suited. Here, too, it pays to heed the nature of institutions and to allow them to perform those jobs for which they are best suited.

Mr. **Fuqua**. Our last witness is Professor Daniel Kevles, Department of Humanities and Social Sciences at the California Institute of Technology.

### STATEMENT OF DR. DANIEL J. KEVLES, PROFESSOR, DEPARTMENT OF HUMANITIES AND SOCIAL SCIENCES, CALIFORNIA INSTITUTE OF TECHNOLOGY, PASADENA, CA

Dr. **Kevles**. Thank you very much, Mr. Chairman and members of the task force.

For the record, Mr. Chairman, you are among legion numbers of people who mispronounce my name. That includes professors, university presidents, secretaries, telephone operators, and so on.

Mr. **Fuqua**. I am familiar with that. [Laughter.]

Dr. **Kevles**. I have prepared a written statement which I believe you have before you, but in the interest of brevity, I have abbreviated it.

Mr. **Fuqua**. We will make it part of the record in its entirety.

Dr. **Kevles**. Right. I have abbreviated it somewhat in the interest of time, and so you will have to skip from place to place.

Now, let me say that I am an historian, and I take it as my task, I am best qualified for here, to try to bring some perspective to this issue, rather than to advance particular prescriptive ideas as to how to resolve it, though I do have some.

Let me therefore begin by calling your attention to two statements addressed to the issue of science in the political process, one made by a very distinguished American geologist, the other by a Secretary of the Navy.

The geologist declared that the control of science must be kept out of the hands of what he called officers or functionaries whose principal interest is, again in his words, "official position or dignity." He added that the great body of scientific men would not tolerate the promotion of research under a predominantly, and I quote again here, "political institution."

The Secretary of the Navy held that research policy, in contrast, was determined by a political process that in his view was properly the instrument of democratic decisionmaking. In this regard, he insisted, the agencies of Government were inevitably political, and they incorporated scientific bureaus only so that they might better perform their political functions. To make them autonomous, in his view, would wrongly free them from the obligation of carrying out their democratically defined purposes.

Now, these statements may have a familiar, contemporary ring, and we have heard echoes of them here this morning. But they were not made in the 1980's; they were made a century ago in the 1880's during the course of the Government's first full-scale investigation of Federal scientific research.

The investigation, which began in 1884 and lasted until 1886, was conducted by a bipartisan Joint Commission of the House and Senate under the chairmanship of Senator William B. Allison of Iowa, one of the most powerful members of that body at that time.

Federal science had grown enormously in the 20 years after the Civil War. Critics of the rapid growth complained that the United

States was investing more in science than all the countries of Western Europe combined, and I think that may have been true.

But whatever the truth, Federal science had become big enough to notice, big enough to provoke debate over how and by whom it was to be controlled, big enough to stimulate consideration of whether its purpose and execution were to be determined by laymen acting through the political process or by scientists insulated from the political process and acting autonomously in accord with their own expert judgment. They meant peer review, although the phrase had not yet been invented.

Now, debate over science in the political process thus, by this simple and limited illustration, has a long history in the United States. The debate has been marked by numerous twists and turns, but it seems to have pivoted persistently on two major issues. The first has centered on the programmatic control of Federal science—that is, who should determine what kind of science should be done and how —the second, on the distribution—geographical and institutional—of Federal investments in research.

American scientists have traditionally, all the way to the 1880's and since, desired to keep control of Federal science as much as possible to themselves, to insulate it from the hands of politics and politicians. To many scientists, politics gave laymen the power, they worried, to establish the programmatic scope of Federal research, to assess the merits of its execution, even to control the dissemination of its results. They feared that the line between informed lay management and know-nothing restrictionism was by no means sharp.

They have adamantly contended, as the American Association for the Advancement of Science resolved during the proceedings of the Allison Commission a century ago, that scientific work is very best judged by scientific people. And they have preferred direct governmental support for science, with only nominal governmental control. They thought that scientists should be responsible to government, but not responsive to dictates as to what fields of science should be investigated or how.

Up to World War II, however, in general, Congress and the executive branch insisted for the most part upon the subjection of Federal science to political control and politically determined purposes; in the best sense, that politics is the instrument by which the people of this country come to some agreement about what policy should be.

The distributive issue did not surface until well after the turn of the century. At the time, almost all federally supported research was conducted in the Government's own agencies. Pressures for change began to emerge after World War I, which dramatized the importance of the physical and laboratory sciences to both national security and national economic competitiveness on an international scale.

During the interwar years, various measures were proposed for Federal programs of assistance to research in the physical sciences at the Nation's colleges and universities. However, the scientific and engineering community was split over how the funds should be distributed.

Spokesmen for State universities and land-grant colleges, pointing to the precedent of the Morrill Act—which, as you no doubt know established the land-grant college system on a State-by-State basis—urged that the funds be distributed on a State-by-State basis and, in an early version of these measures, within each State, to a specifically designated public institution.

In contrast, leading scientists—that is, those from the very best universities, who were among the scientific elite in their day—insisted that the funds should be distributed on the basis of scientific merit, and this was to be determined by some version of peer view, to projects and investigators wherever they might be located.

Proponents of this method held that awarding funds to the most meritorious projects was the most efficient way to foster productive research. Karl Compton, the president of MIT, explained to a congressional committee in 1937 that supporting research on a routine geographical basis resembled, in his words, "the firing of broadsides in a general direction," whereas funding meritorious research projects was like "firing a sharpshooter directly at a target."

However, funding the most meritorious projects meant concentrating the funds in the relatively small number of well-equipped institutions where the better scientists and engineers were located. And since those institutions tended to be concentrated by the mid-1930's already in the Northeast, the Midwest, and California, the result would be a geographical concentration of Federal research funds.

Not surprisingly, the advocates of the meritorious project approach came from institutions in those regions, while the proponents of a geographical distribution of funds did not. Each faction was sufficiently powerful politically to keep the other from prevailing legislatively and, in effect, to prevent the establishment of any Federal program of research in the physical sciences at the Nation's colleges and universities.

It is well known that World War II forced a sharp change in this state of affairs. Considerations of national security and national welfare demanded that the Federal Government assume responsibility for ensuring to the country a scientific and engineering enterprise second to none, particularly through support for research and training in the country's colleges and universities.

Perhaps not so well known—or at least long forgotten now—is that there was considerable dispute at the end of World War II as to just how these goals were to be accomplished. Vannevar Bush's celebrated report, "Science, the Endless Frontier," advanced only one means of achieving them. An alternative was proposed by Senator Harley M. Kilgore of West Virginia, whose State, of course, at that time did not include a scientifically distinguished university.

The Kilgore plan provided for political control of the Nation's scientific program and a degree of geographical equity in the distribution of research funds. I might add to Congressman Walgren that by political control, Kilgore intended to establish systems of peer review that included laymen, people interested in social programs, as well as professional scientists.

The Bush plan, in contrast, sought to insulate Federal science as much as possible from political considerations and to distribute research funds to the best scientific projects, even if that led to geographical and institutional concentration of these moneys.

Thus, at the end of World War II, the longstanding issues of both the control and distribution of Federal largess for science came to a head. In the end, Bush's approach won out on both counts as, I might stress, a political decision, not some decision made by some dictator but a decision of the Congress in consonance with the President.

Bush's approach won out on both counts, as I say, though not without some modification on the control issue, since President Truman made clear in his 1947 veto of the first bill to create a National Science Foundation that would have followed Bush's lines of organization that he, the President, would not tolerate the establishment of a national science policy entirely insulated from the control of the President.

In the 20 years or so after 1945, the Nation's scientists operated with considerable autonomy in the shaping of national research policy, and Federal research funds, distributed in accord with the judgments of peer review to the best qualified projects, tended inevitably to concentrate geographically and in a relatively small number of institutions.

But by the mid-1960's, by which time Federal expenditures for R&D had risen to about $15 billion a year, there was growing dissatisfaction with the degree to which national science policy seemed unresponsive to the political expression of social needs and kept producing what, to the have-nots, appeared to be an inequitable distribution of research dollars.

For these reasons, in 1963, this House of Representatives ordered a full-scale investigation into federally funded science. It was the first since the Allison Commission in the 1880's. Other investigations followed in the 1960's. These inquiries did produce some modification in the national system of science, making it politically more responsive and compelling it to spread R&D funds more widely. This—in following Dr. Rosenzweig's testimony—is why some of the distributive effects have changed and been spread more widely in the last 20 years.

However, the existence of this task force today indicates that the fundamental issues of science in the political process have by no means been entirely resolved. The fact that the issues of control and the distribution of Federal largess for science have such a long history suggests that they may not be resolved easily or quickly, if ever, entirely to everybody's satisfaction.

There is simply, I would suggest, a fundamental tension between, on the one side, the requirements of high quality science and, on the other, the broader needs of the national, regional, or local socioeconomic and educational welfare. The demands of both seem to me to be legitimate, and I have no special wisdom to propose about how to adjudicate between the two.

I would suggest, however, that it is important for policymakers in the Congress and the executive branch not to deceive themselves into assuming that the pursuit of one aim will efficiently and necessarily yield the other as well. Policies designed to achieve the highest quality science are not, on the average, likely, at least in the short term, to serve a geographically or institutionally broad-based socioeconomic interest.

Alternatively, policies constructed to advance education, employment, and the like will not necessarily produce the best possible science, again in the short term. What we have, then, is somewhat overlapping but, in fundamental respects, conflicting interests. The weight to be given to each is unavoidably a matter for political judgment and decision through the political process.

But an important distinction here must be kept in mind. There is politics as the instrument to shape policy by resolving the play of conflicting or disparate interests. There is also politics as the unalloyed exercise of power and influence, unconstrained by considerations of policy and merit. The former, it seems to me, certainly has its place in Federal science. The latter, it seems to me, should be scrupulously avoided to the best possible extent.

Thank you. I will be glad to elaborate.

[The prepared statement of Dr. Kevles follows:]

# SCIENCE IN THE POLITICAL PROCESS
# HISTORICAL REFLECTIONS

Daniel J. Kevles

Let me begin by calling your attention to three statements addressed to the issue of science in the political process, one made by a very distinguished American geologist, the other two by, respectively, a member of the House of Representatives and a Secretary of the Navy.

The geologist declared that the control of science must be kept out of the hands of "officers or functionaries" whose principal interest is "official position or dignity," and he added that the great body of scientific men would not tolerate the promotion of research under a predominantly "political institution"

The member of the House declared that he saw "no reason why a scientist . . . should be more competent than a Congressman to say how much topography a sailor would want on his chart. If it were simply a question as to the best method of obtaining absolute exactness, the judgment of a pure scientist would outweigh the opinion of the man of affairs; but not so when the question is as to the adaptability of the work accomplished to the purpose of the law." The Secretary of the Navy held that such adaptability and purpose — that is, research policy — were determined by a political process that was the instrument of democratic decision-making. In this regard, he insisted, the agencies of government were inevitably "political" and incorporated scientific bureaus only so that they might better perform their political functions. To make them autonomous in their activities would wrongly free them from the obligation of carrying out their democratically defined purposes.

These statements may have a familiar, contemporary ring, but they were not made in the 1980s. The geologist was John Wesley Powell; the Congressman, Hilary Abner Herbert of Alabama; and the Secretary of the Navy, William B. Chandler. They made their statements a century ago, in the 1880s, during the course of the government's first full-scale investigation of federal scientific research.

The investigation, which began in 1884 and lasted until 1886, was conducted by a bipartisan Joint Commission of the House and Senate under the chairmanship of Senator William B. Allison of Iowa. It was prompted by a variety of concerns, including charges that some of the federal scientific agencies were corrupt and that others were exceeding their mandates by engaging in types of research that they were not authorized to do. But there was a more fundamental issue behind the Congressional interest. Federal science had grown enormously in the twenty years after the Civil War. The period was marked by considerable expansion in the work of the venerable Coast and Geodetic Survey; by the establishment of the United States Weather Service in the Army Signal Corps; and by the creation and rapid development of the United States Geological Survey under the leadership of John Wesley Powell. The federal budget for science had grown to more than half a million dollars a year, which nowadays, of course, is scientific small change, but which at the time was more than a half percent of the federal budget. Critics of the rapid growth complained that the United States was investing more in science than all the countries of western Europe combined. Whatever the truth, federal science had become big enough to notice, big enough to provoke debate over how and by whom it was to be controlled, big enough to stimulate consideration of whether its purpose and execution were to be determined by laymen acting through the political process or by scientists insulated from the political process and acting autonomously in accord with their own expert judgment.

Debate over science in the political process thus has a long history in the United States. The debate has been marked by numerous twists and turns, but it seems to have pivoted persistently on two major issues: the first has centered on the programmatic control of federal science — that is, what kind of science should be done, and how; the second, on the distribution -- geographical and institutional -- of federal investments in research.

American scientists have traditionally desired to keep control federal science as much as possible to themselves, to insulate it from the hands of politics and politicians. In John Wesley Powell's day, they feared interference from power-hungry or illiterate officeholders. Then and since, they have contended, as the American Association for the Advancement of Science resolved during the proceedings of the Allison Commission, that scientific work is best judged by scientific men. To Powell and his successors, politics gave laymen the power to establish the programmatic scope of federal research, to assess the merit of its execution, even to control the dissemination of its results. They feared that the line between informed lay management and know-nothing restrictionism was by no means sharp. They therefore preferred direct governmental support for science with only nominal governmental control. They thought that they should be responsible to government, but not responsive to dictates as to what fields of science should be investigated or how. Up to World War II, however, Congress and the executive branch insisted, for the most part, upon the subjection of federal science to political control and politically determined purposes.

The distributional issue did not surface until well after the turn of the century. Before then, federal research was concentrated in the earth sciences, e.g., fields such as geology and agriculture, which were relevant to the exploration and settlement of the country. Furthermore, almost all federally supported research was conducted in the government's own agencies. With one programmatic exception, the government did not subvene science in the nation's colleges and universities. The exception was the funds it provided on a state-by-state basis to agricultural experiment stations. same amount of money. Pressures for change began to emerge after World War I, which dramatized the importance of the physical and laboratory sciences to both national security and national economic competitiveness. During the interwar years, various measures were proposed for federal programs of assistance to research in the physical sciences at the nation's colleges and universities. However, the scientific and engineering community was split over how the funds should be distributed.

Spokesmen for state universities and land-grant colleges, pointing to the precedent of the Morrill Act, urged that the funds be distributed on a state-by-state basis, and in an early version of these measures, within each state to a specifically designated public institution. In contrast, leading scientists insisted that the funds should be distributed on the basis of scientific merit — this would be determined by some version of peer review — to projects and investigators wherever they might be located. Proponents of this method held that awarding funds to the most meritorious projects was the most efficient way to foster productive research. Karl Compton, the president of M.I.T., explained to a Congressional committee in 1937 that supporting research on a "routine geographical basis" resembled "the firing of broadsides in a general direction," whereas funding meritorious research projects was like "firing a sharpshooter directly at a target."

However, funding the most meritorious projects meant concentrating the grants in the relatively small number of well-equipped institutions where the better scientists and engineers were located. And since those institutions tended to be concentrated by the 1930s in the Northeast, the Midwest, and California, the result would be a geographical concentration of federal research funds. Not surprisingly, the advocates of the meritorious-project approach came from institutions in those regions, while the proponents of a geographical distribution of funds did not. Each faction was sufficiently powerful politically to keep the other from prevailing legislatively — and in effect to prevent the establishment of any federal program of research in the physical sciences at the nation's colleges and universites.

It is well known that World War II forced a sharp change in the state of affairs. Considerations of national security and national welfare demanded that the federal government assume responsibility for ensuring the country a scientific and engineering enterprise second to none, particularly through support for research and training in the country's colleges and universities. Perhaps not so well known is that there was considerable dispute at the end of World War II as to just how these goals were to be accomplished. Vannevar Bush's celebrated report, Science, The Endless Frontier, advanced one means of achieving them. An alternative was proposed by Senator Harley M. Kilgore of West Virginia. The Kilgore plan provided for political control of the nation's scientific program and a degree of geographical equity in the distribution of research funds, while the Bush plan sought to insulate federal science as much as possible from political considerations and to distribute research funds to the best scientific projects, even if that led to geographical and institutional concentration of these monies.

Thus, at the end of World War II, the longstanding issues of both the control and distribution of federal largesse for science came to a head. In the end, Bush's approach won out on both counts (though not without some modification on the control issue, since President Truman made clear in his 1947 veto of a bill to create a Bush-like National Science Foundation that he would not tolerate the establishment of a national science policy entirely insulated from the control of the President). In the twenty years or so after 1945, the nation's scientists operated with considerable autonomy in the shaping of national research policy, and federal research funds, distributed in accord with the judgments of peer review to the best qualified projects, tended to concentrate geographically and in a relatively small number of institutions.

But by the mid-1960s, by which time federal expenditures for R & D had risen to about $15 billion dollars a year, there was growing dissatisfaction with the degree to which national science policy seemed unresponsive to the political expression of social needs and kept producing what to the have-nots appeared to be an inequitable distribution of research dollars. For these reasons, in 1963 the House of Representatives ordered a full-scale investigation into federally funded science, the first since the Allison Commission in the 1880s. Other investigations followed in the 1960s. These inquiries produced some modification in the national system of science, making it politically more responsive and compelling it to spread R&D funds more widely. However, the existence of this Task Force suggests that the fundamental issues of science in the political process have by no means been resolved entirely.

The fact that the issues of control and the distribution of federal largesse for science have such a long history suggests that they may not be resolved easily or quickly, if ever. There is simply a fundamental tension between, on the one side, the requirements of high-quality science and, on the other, the broader needs of the national, regional, or local socioeconomic and educational welfare. The demands of both seem to me to be legitimate. I have no special wisdom to propose about how to adjudicate between the two. I would suggest, however, that it is important for policymakers not to deceive themselves into assuming that the pursuit of one aim will efficiently yield the other as well. Policies designed to achieve the highest quality science are not likely, at least in the the short term, to serve a geographically or institutionally broad-based socio-economic interest. Alternatively, policies constructed to advance education, employment, and the like will not necessarily produce the best possible science. What we have then is somewhat overlapping but in fundamental respects conflicting interests. The weight to be given to each is unavoidably a matter for political judgment and decision through the political process.

## DISCUSSION

Mr. **Fuqua**. Thank you very much.

As an historian, are you familiar with any comparable cases in other fields such as medicine or law or defense policy, social security or welfare policy, where the balance between professional judgment and political judgment has been at issue?

Dr. **Kevles**. Yes. The major area that is familiar to me is in regulatory policy, going all the way back to, well, the Interstate Commerce Commission in the 1880's, the establishment of the Food and Drug Administration in the early 20th century.

A more recent case would have been in the 1950's with the so-called battery additive case, in which the National Bureau of Standards certified that a battery additive did nothing to add any life or strength to commercially available batteries, and the manufacturers of these batteries and their allies in the Congress tried to overturn this decision and tried to have the director of the National Bureau of Standards fired.

In the end, the scientific community rallied around this kind of expert judgment, and the Congress and the executive branch, then under the control of President Eisenhower, backed down.

But there have been many cases, the details of which don't readily come to my mind, but you can imagine what they must have been in the regulatory process as between the judgments of scientific experts and the economically interested manufacturers of various products.

I don't know, offhand, of issues, however, comparable to the distributive one in other fields as to the distribution of research moneys. The case in agriculture, which you are probably familiar with, the Federal Government established under the Hatch Act in 1887 an agricultural experiment station in every State, with the munificent appropriation of $15,000 a year to each.

On top of this, however, there was added the Adams Act in, I believe, 1906, which did allocate funds more for basic research to somewhat of a degree on a project basis. But the distribution of funds to agriculture was largely geographical.

Mr. **Fuqua**. Of course, agriculture today is still generally funded on a formula basis.

Dr. **Kevles**. That is right, but I think Dr. Rosenzweig is right, that we have a very limited number of studies as to the effectiveness of research funding in terms of output, productivity and quality. We do have some impressions, however, of the overall effectiveness of funding in agriculture.

It may not surprise you that at least my impression is that where the agricultural experiment stations have been allied with first-class universities, or universities that historically chose to make themselves first class—for example, the University of Wisconsin—they have been outstanding long before any other kind of Federal support came to these universities.

But where they were not allied with first-class research facilities and made no attempt to transform themselves into those, they are very, very low quality and low productivity.

I think one of the important points to recognize is that what we know statistically about the productivity and quality in science strongly suggests that a disproportionally large amount of significant work in science, engineering, and what have you is produced by a disproportionally small amount of the scientific community, even where they are relatively equally funded, even in good institutions. And so one must take this into account, among many other things, in developing research policy for funding.

Mr. **Fuqua**. Mr. Walgren?

Mr. **Walgren**. If a disproportionally small number are producing a disproportionally large amount of the good research, as I am sure is the case, that seems to make it more striking that we are sort of unable to predict. As you say, there is the impression that those aligned with universities did better, but as I am trying to remember what you just said, I guess I am struck by the difficulty of predicting the value of the research that would follow on. Is that a fair statement of your remarks?

Dr. **Kevles**. No; I didn't intend to imply that. What I meant to say is that the fact of the matter is that a very large amount of significant work is produced by a very small number of people. The question is how you go about identifying those people when they are young, in particular, and with regard to where they are.

We have a lot of indices by which to measure those things. They are, I am sure, all rolled up into the sorts of questions that people ask on those site visits that Dr. Rosenzweig mentioned, and they have to do with the quality of their training, the nature and the merits of what they propose to do, with the general personal impression they give of drive, ambition, knowledgeability about the field and so on.

Mr. **Walgren**. But I thought I heard you say that we don't have very good studies which can predict these things——

Dr. **Kevles**. That is right.

Mr. **Walgren** [continuing]. And we are a little bit operating on impression.

Dr. **Kevles**. That is right, but those studies we do have tell us strongly and explicitly that a very small number of people are largely significant.

Mr. **Walgren**. Yes; that is right, but the studies themselves are not studies that——

Dr. **Kevles**. They are post-hoc studies as to where—you look at the productivity and see who did it.

Mr. **Walgren**. All right. So, not disputing the conclusion that the small number produce a large amount, is it your view of our present abilities to predict who will produce that and where it will be produced are relatively limited?

Dr. **Kevles**. Well, let me clarify that. I think they are not totally limited. Let me pose a case in extremis. You have driven across the country a number of times as I do, living in California. You know that there are vast reaches of land out in the West beyond the 100th meridian that are very limited in population, and some towns are just crossroads and a gasoline station.

Now, suppose someone from a crossroads and gasoline station proposed a biomedical research facility. Well, of course, that would be a ludicrous proposition, because the staff is not there to use it, the support facilities and so on are not there.

Where you have institutions, however, that are closer to takeoff and you can measure how close they are to that, then it seems to me that you have strong opportunities and justification for making investments in facilities; and then where, of course, these things already exist, you know that they will work.

The point I wanted to make in closing, at the end of my remarks, is that the development of scientific facilities, of new centers for training and so on, seems to me a highly legitimate thing for the Congress, the Government of the United States, to be concerned with, and considerations of geographical equity, of economic development and so on, seem equally legitimate to me.

You must recognize, however, that in so doing you are likely to get less scientific return for the dollar in the short term than you are investing the same dollar, say, in 1 of the 20 to 50 or so major research institutions in the United States as they currently exist.

You should not confuse the two aims. You should recognize that both are legitimate. It is a matter of political judgment as to how much weight to give to both and how many dollars to invest in both.

Mr. WALGREN. What criteria did they use in the sixties when they apparently had a pretty full facilities program? Dr. Rosenzweig said they asked themselves whether an area was likely to produce competitive research. What kinds of professional judgments and competitive peer reviews went into that program?

Dr. KEVLES. Well, I am not familiar with the details of how that worked, but I can say I do know for sure that concerns arose and were expressed and found expression in the Congress as to the inequitable distribution, geographically and institutionally, of research funds, and it was in response to that that the Congress moved in the direction of establishing a policy—and so did President Johnson—to try to enlarge the base of first-class scientific research institutions.

Now, the important point is, however, that this process of spread, which I think was successful and has worked —I mean, if you look at the number of first-class research institutions in the United States, it has grown steadily ever since the late 19th century, and it has grown since the 1950's. This is a very important thing to notice. It is not a fixed and static group that has controlled the door to the candy shop.

But the process by which this enlargement was accomplished was a process of mutual negotiation among the agencies, the scientific community, those institutions that were aspirant to improve themselves, and the Congress. It is not as though these different constituencies should be at war with each other.

What I think I object to, and was alluding to at the very end of my remarks, is that in the absence of a congressional policy to enlarge the base and to establish criteria as a policy matter as to how you want to enlarge the base and to what degree and for what purpose, that simple sort of logrolling kinds of amendments to appropriations bills or authorization bills, while certainly within the right of the Congress, would seem to me to be unwise in terms of serving an overall national policy of scientific development. That is where, it seems to me, mistakes have been made in recent years.

Mr. WALGREN [as Acting Chair]. Thank you.

**[Answers to questions asked of Dr. Kevles follows:]**

CALIFORNIA INSTITUTE OF TECHNOLOGY

DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES 228-77

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COMMITTEE ON SCIENCE
AND TECHNOLOGY

August 12, 1985

Congressman Don Fuqua
Chairman
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Congressman Fuqua:

Enclosed are the responses I find myself able to supply to the "Questions for the Record" posed in your letter of July 10.

Let me take the liberty of bringing up a subject that seems to me to be quite relevant to the nature of the inquiry into federal science policy that your Committee on Science and Technology is now conducting. This is the level of funding for research in the history of science provided by the National Science Foundation.

Much of what we know about the history of federal science policy and how the growth of science and technology has been fostered in the United States has come from studies supported during the last twenty years or so by the NSF's history of science program. This includes some of the work that I was able to consult in preparing my testimony for your Committee. However, during the first year of the Reagan administration, drastic cuts were made in the budget of this program, and it has by no means fully recovered the ground lost. I do not have the exact figures available, but I believe that the program's current annual budget -- about $1.6 million -- is significantly smaller in constant dollars than it was in the late 1960s. In order to spread its limited funds as widely as possible among qualified scholarly projects, the program has limited the maximum grant available to an individual investigator to $39,000. Relatively few of these grants can be obtained, and the dispensable amount, once institutional overhead has been subtracted, does not go very far in defraying the costs of a major research project.

Historical studies of science and technology have a good deal to contribute to the shaping of sound federal policy in the area. The making of policy may be formed from good judgment anchored in knowledge of how science and technology work in relationship to each other and to the larger society. Such knowledge may be gained from well-considered personal experience, but even more so from historical and sociological studies, which can identify effective connections among events and forces of which the historical or contemporary actors were unaware. Such studies not only reveal the sources and consequences of policy decisions in the past; they can also advantageously clarify policy alternatives and costs in the present. (The current debate about the proposed super-conducting super collider, for example, would profit greatly from a consideration by historians of the political economy of particle accelerators since the 1950s.) More generally, such studies go behind corporate and cultural myths to illuminate how science and technology work, and to expose the relations among scientific advance, industrial, development, and the socioeconomic impact of science and technology.

Given the immense importance of federal science policy, greater investment in producing the type of knowledge that would assist in its sound formation would seem to be wise. The cost of doing the history of science and technology is not large; for example, it would take only about $1.6 million a year to double the budget of the NSF program in the history and philosophy of science. Funding studies in the field might well be considered a sort of tax — and a very small one at that — on the huge federal R&D budget. The price would be tiny, but, since the potential gain in understanding would be very large, it seems to me penny-wise and pound-foolish for the government not to pay it.

I therefore strongly urge you to look into the matter and hope that you will see fit to attempt to do something about increasing the funds available for research in the history of science.

Best wishes,

Yours sincerely,

Daniel J. Kevles
Professor of History

## QUESTIONS AND ANSWERS FOR THE RECORD

Dr. Daniel J. Kevles

1. **The recent cases of attempts to obtain science facilities for individual universities through the political process are not the only instances where conflict between political and scientific judgments have taken place. We have also seen, for example, attempts by the Congress to establish new, mission-oriented institutes at the National Instututes of Health (arthritis, nursing) which the medical research community opposed. Do you see any generally applicable lessons that can be drawn from all of these cases?**

   Obtaining special facilities through the political process is different from using the political process to establish new mission-oriented institutes in existing federal agencies. The first aims purely at producing a local benefit; the second constitutes a programmatic initiative. Nevertheless, both may well violate a long-standing principle of federal policy for science -- namely, that research investments should be made in a matter that produces the highest return per dollar in new basic or usable knowledge. This is possible in the first case, since research facilities located at scientifically weaker institutions are unlikely, at least in the short term, to yield as much research of high quality as those placed at the scientifically strongest institutions. A similar possibility arises in the second case in that, given the current state of fundamental knowledge concerning disease, the investment of funds in developing therapies for particular diseases may prove to yield only very limited returns. I see no general lesson to be drawn from these two cases, but they do seem to throw into bold relief a challenge to the Congress -- that is, how can the Federal Government satisfy legitimate particular needs (whether defined geographically or in terms of special groups, e.g., those who suffer from one or another disease) while maintaining one of the long-standing principles on which federal science policy has been based. As to how the challenge might be met, see #2, below.

2. **One of the traditional rules of the political world, arising from the concept of representational government, is that legislators have a duty to look out for the interests of their constituents. This a particularly strong tradition in our American system where we have no members elected "at large" or on the basis of total party votes, but all represent specific geographic areas and the people living within that area. Should we expect legislators in such a system to exempt certain parts of the Federal Government's activities, be it science, or defense, or the arts, or any other field, from such political interest or influence?**

   We should not expect legislators to exempt certain parts of the Federal Government's activities from political oversight. However, while local interests should, of course, be represented in government, politics should be the instrument by which their needs are melded into sound public policy. Politics should not ordinarily be the means by which they simply aggrandize themselves on an ad hoc basis (e.g., by obtaining research facilities through unalloyed political pressure). If Congress wishes to change or modify the policy by which money for research facilities is distributed, then it should do so openly and explicitly, not through the backdoor.

Congress could well decide to supplement existing policy with a program to aid weaker institutions in their ambitions to become first-rank ones. However, it would be wise in doing so to impose criteria for the administration of such a policy that are related to its stated aim -- i.e., not criteria of political influence but of sufficient scientific merit to benefit from the additional investment in facilities. Similarly, if Congress desires to aid people who suffer from particular diseases, it should do so in a way that is consistent with real, not wishful, scientific possibility. (It would have been fruitless, for example, for Congress to have created a crash program in 1920, or even in 1930, for the controlled release of nuclear energy in sufficient quantities to act as a power source. That had to await the development of nuclear physics and the discovery of fission.)

3. **If certain aspects of federal activity, such as science, are exempted from political determination, what happens to the concepts of political accountability for the individual legislator?**

As above, I don't believe that any aspect of federal activity should be exempted from political accountability. The point is to establish policy for the activity through the political process, then abide by it. If it proves unsatisfactory, then change it, but don't undercut it while it is in place through political interference.

4. **It has been argued that one reason why some universities have sought to obtain assistance through the political process is that the peer review process is too heavily weighted in favor of the top institutions because it is operated by an "old boy network". Thus, it is said, those who are not part of the establishment can only obtain a share of the available resources by going outside the peer review system. Are there any specific data to either support or refute this? If it is true to any extent what is good and bad about going through the political process?**

I don't have enough hard data to respond to this question. However, even under the best of circumstances and good will, the peer review process will work to the advantage of the better institutions and scientists, even with no old-boy favoritism. The issue is the long-standing principle mentioned in #1 above and how to deal with its effects, discussed in #2 above.

5. **In the event we continue to see the practice of lobbying for research facilities or other attempts to make judgments about science within the political process at about the level of the past 3 to 4 years, what, if any, will the long-term effects be, in your opinion? Is there a threshold level where this practice has serious adverse effects, in your view?**

In my opinion, there would be serious adverse effects over the long term on the quality and vitality of American science if the distribution of research funds and facilities were -- to take an extreme case -- made entirely subject to the same political process as, say, rivers and harbors appropriations. The fact of the matter is that, in most times and places, a relatively small number of people at a small number of institutions have been responsible for a disproportionately large fraction of scientific progress. Invest in them, and you get a high return for your money. Spread the funds evenly, and you will likely have the gratitude of diverse political constituents but a weakened research enterprise.

**Mr. WALGREN.** The task force will adjourn until 8:30 tomorrow morning. We appreciate your being available to us.

[Whereupon, at 12:49 p.m., the task force recessed, to reconvene at 8:30 a.m., the following day, Thursday, June 27, 1985.]

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